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[*Continued on page (III) of Cover*

THE MEASUREMENT OF NOISE, WITH SPECIAL REFERENCE TO ENGINEERING NOISE PROBLEMS.

By B. G. Churcher, Member, A. J. King, B.Sc.Tech., Associate Member, and H. Davies, M.Eng., Graduate.

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Summary.

The paper deals with a detailed investigation of noise measurement for the purpose of establishing methods generally applicable to the sustained noises encountered in engineering. The work follows on that described in a previous paper. The necessity for redetermining the properties of the hearing system of the average individual in terms of free-space conditions of listening is explained, and the results obtained on 50 persons are given. The inadequacy of the decibel scale for indicating subjective loudness is discussed and two alternatives are considered. The second of these is suggested as being the more helpful in conveying numerically the desired impression of loudness.

Desirable improvements in the noise-analysing apparatus described in the previous paper are indicated, and a new equipment embodying these features is briefly described.

The various methods of assessing the loudness of a complex noise are considered in relation to engineering requirements, and the reasons are given for the choice of the aural balance method using a pure reference tone with a frequency of 800 cycles per sec. Experience obtained with this method in assessing typical noises is described. Finally, consideration is given to the problem of specifying the noise which a piece of apparatus may make when installed in a particular location.

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(1) Introduction.

Before discussing noise measurements a necessary preliminary is to define what we mean by noise. The old conception was that sounds which occupy the attention of a person could be broadly classified into music and noise. This classification was based solely on the characteristics of the stimulus, i.e. on physical quantities such as the relations between the component frequencies. This view is now seen to be untenable. Subjective considerations are at least equally important. For the purposes of this paper we define noise as undesired or irksome sound. For example, a raucous motor horn presents no difficulty, the sound being noise on both older and newer definitions. The hum of a transformer, however, complies with the old physical definition of music; but few people would classify the sound as anything but noise.

Again, the sound from a wireless set operated in a room to the evident pleasure of certain individuals may constitute an irksome noise to others who wish to converse.

Lastly, many if not most persons would unhesitatingly classify certain modern orchestral compositions as noise, while those with sufficiently developed musical appreciation would classify them as music.

During the early stages of the investigation, described in a previous paper,* the authors relied on the curves of the minimum and maximum audible intensities of the average ear obtained by R. L. Wegel,† of the Bell Telephone Laboratories. By calling these curves zero and 100 loudness units respectively, and dividing the intervening range into 100 logarithmic intervals in accordance with the Weber–Fechner law, a scale for the interpretation of the r.m.s. pressures of pure tones in terms of proposed units of sensation was evolved. This scale covered the range 50 to 5 000 cycles per sec. and gave a much better perspective of the relative importance of the various components of a noise than was given by leaving the results of an analysis in terms of acoustical pressures. For some months it was very helpful in developing quiet-running machines by indicating the best line of attack, but, as machines were made quieter, anomalous results were obtained. For example, a low-frequency component which was quite definitely audible would appear on the loudness scale as below zero. It was therefore clear that the curve of minimum audibility did not correspond to the conditions of test. Wegel's results apply to measurements made with a telephone held to the ear, whereas the measurements of the present authors are of sound-field pressures under approximately free-space conditions, i.e. corrected for the presence of the measuring microphone and in the

* *Journal I.E.E.*, 1930, vol. 68, p. 97.

† R. L. Wegel: "Physical Characteristics of Audition," *Bell System Technical Journal*, 1922, vol. 1, p. 56.

absence of the observer. Wegel's results therefore do not take into account the collecting power of the outer ear and possibly of the bones of the head. So far as the authors are aware, no satisfactory information was available on this point, and an extensive series of measurements was therefore undertaken to determine the corresponding threshold curve for free-space conditions. As regards the curve of maximum audibility, the experimental difficulties in the way of producing pure tones of such intensities in free space are almost insuperable. For this and other reasons the loudness scale based on the upper and lower limits of hearing was abandoned and, for the time being, recourse was had to the method—outlined on page 122 of the previous paper*—of expressing the intensity of a note in terms of the number of decibels above the threshold intensity. It was obvious from previous work with the loudness scale that the decibel figures for different frequencies would not be directly comparable, and that it would be necessary to determine by direct comparison the relation between the decibels above threshold of equally loud notes at different frequencies in order to establish a common basis for loudness measurement. Another reason for the reconsideration of the basis of the noise measurements was that the relative intensities of two notes of different strengths, expressed in decibels above threshold, seemed in many cases to diverge considerably from mental estimates of their relative loudnesses. Thus when comparing 800-cycle notes of, say, 90 and 45 decibels above threshold, the former seemed much more than "twice as loud" as the latter. Apparent discrepancies of this kind were not only noticed by the authors and their colleagues but were the subject of criticism from sources which, from an acoustical point of view, could be regarded as non-technical. If these discrepancies are real, the inference is that the Weber-Fechner law is only an approximation and that the deviation between the decibel and sensation scales is sufficient to be of practical importance. Attention has been called to this point by several investigators who will be referred to later. As this matter is of such a fundamental nature, a detailed investigation became necessary.

In attempting, therefore, to formulate a basis for noise measurement which would be acceptable for engineering purposes, the authors have considered first the laws of response of the hearing system of the average individual to sounds of different characteristics, and then the choice of a practical method of noise measurement in accord with those laws. The investigation of the laws of hearing falls into three sections, viz. the determination of the threshold of hearing, which provides a datum from which intensity levels may be reckoned, the determination of the relation between the magnitudes of stimuli of different frequencies which produce equal loudness sensations, and finally the relation between stimulus and sensation.

(2) The Characteristics of the Hearing System.

(a) *The Determination of the Threshold of Audibility.*

Without further definition the "threshold of audibility" applies to the region between audibility and inaudibility and does not imply a definite criterion such as is necessary in carrying out a series of tests on individuals. Since it was intended that the value, when determined, should constitute the zero of a sensation scale, the most relevant definition seemed to be the most intense sound which is inaudible, the smallest detectable sound clearly calling for a finite measure of sensation.

In order to avoid confusion, the threshold was further defined as the largest sound the complete removal or application of which is not detected. This criterion of the threshold lends itself to a simple experimental procedure which calls for a minimum of effort on the part of the subject. It was also established that, in the case of four skilled observers, the results agreed with the means of their values obtained when approaching the threshold from higher and lower intensities.

When determining the threshold of an individual it is essential that there should be no background noise, as otherwise the de-sensitizing action of the ear operates and raises the threshold. It is also important, when the measurements refer to approximately free-space conditions of listening, that the sound field at the location of the subject should be uniform, any distortion present being due to the subject and not to wall or other reflections. Both these conditions were satisfied by making the measurements in a non-reflecting soundproof test room.*

The range of frequencies covered is from 100 to 6 400 cycles per sec. at octave intervals, so that measurements were made at seven frequencies. Experience has shown that these points are sufficiently close to define the threshold curve.

In order to generate the test tones a moving-coil loud-speaker was suspended near the centre of the room about $1\frac{1}{2}$ metres above the cotton-waste floor-covering and on a level with the head of a subject seated in an elevated chair. Measurements were made at each frequency of the variations in sound pressure likely to be experienced due to differences in height and uncertainties in the positioning of the subject's head. These measurements demonstrated that the variations at the location used, viz. 1·5 metres from the source, did not exceed 1 decibel. Such a small variation is to be expected as, to an object the size of the human head, a wave of 1·5 metres radius appears sensibly plane.

Since the measurements refer to the sensitivity of the ear to pure tones, care was taken to keep the harmonic content of the notes as low as possible. The point becomes of first importance when considering a tone of low frequency, as then, owing to the greater sensitivity of the ear at higher frequencies, a 20-per-cent third or higher harmonic in terms of acoustic pressure may be louder than the fundamental. It is just at low frequencies where it is somewhat difficult to produce pure tones, and it was only after considerable research into the problems involved that a valve oscillator and loud-speaker were produced with negligibly low harmonic output.

In making the measurements the intensity of the test tone from the loud-speaker was controlled in steps of

* *Loc. cit.*

* B. G. Churcher: "The Acoustics Laboratory of the Metropolitan-Vickers Electrical Co.'s Research Department," *Engineering*, 1933, vol. 135, p. 563.

1 decibel by an attenuator, and a switch was provided for removing it entirely. The subject, sitting facing the source, indicated by a signal whether he could hear the note and so enabled an observer to determine in stages the highest intensity not detectable by the subject, when switched on or off.

In order to specify the corresponding sound pressure it was convenient to determine for a given applied voltage the relation between the attenuator setting and the acoustical pressure at a higher intensity, and to demonstrate the linearity of the loud-speaker over this range using a condenser microphone of accurately determined field calibration. It was then a simple matter to deduce from the attenuator-setting the acoustical field pressure in the absence of the subject. Several checks taken during the tests showed that this relation was maintained within 0·5 decibel.

To have measured the threshold of each subject at all seven frequencies at one sitting would have involved errors due to fatigue. The measurements were therefore divided into two groups, namely 100 to 800 cycles per sec. and 800 to 6 400 cycles per sec. This arrangement had two other important advantages. First, the 800-cycle measurement occurs in both series with an interval of several days in between, so that a check is obtained on the consistency of the readings, and secondly the 800-cycle measurement is made at the end of the first series and the beginning of the second series, so that any marked inconsistency due to fatigue would be evident.

In making these measurements a rest period of a few minutes was allowed after each threshold determination before proceeding to the next frequency. The notes were not presented in any rigid order, as repeat tests were interspersed without any indication being given that a check was being made. In very few cases did a subject realize that a tone was being repeated.

In selecting the subjects for the experiments, the object was to obtain data representative of the average person who might be concerned with questions of noise. It was desired to avoid the biasing of the results which would occur if persons either of specially acute hearing or of experience in acoustics were chosen. Only two persons, who were definitely abnormal, were rejected out of the 50 considered for the threshold experiments. Altogether 34 males of ages varying between 15 and 63 years were examined. Among them were engineers, research workers, salesmen, draughtsmen, and artisans. Of these not more than 14 would be expected to adopt a specially critical attitude towards the experiments. The males therefore included persons whose normal working environment ranged from quiet offices to noisy workshop conditions. All except four had no previous experience of acoustical measurements. Of the 14 females, whose ages varied from 15 to 48 years, two only would be expected to adopt a critical attitude towards the experiments, and all were accustomed to work in relatively quiet surroundings.

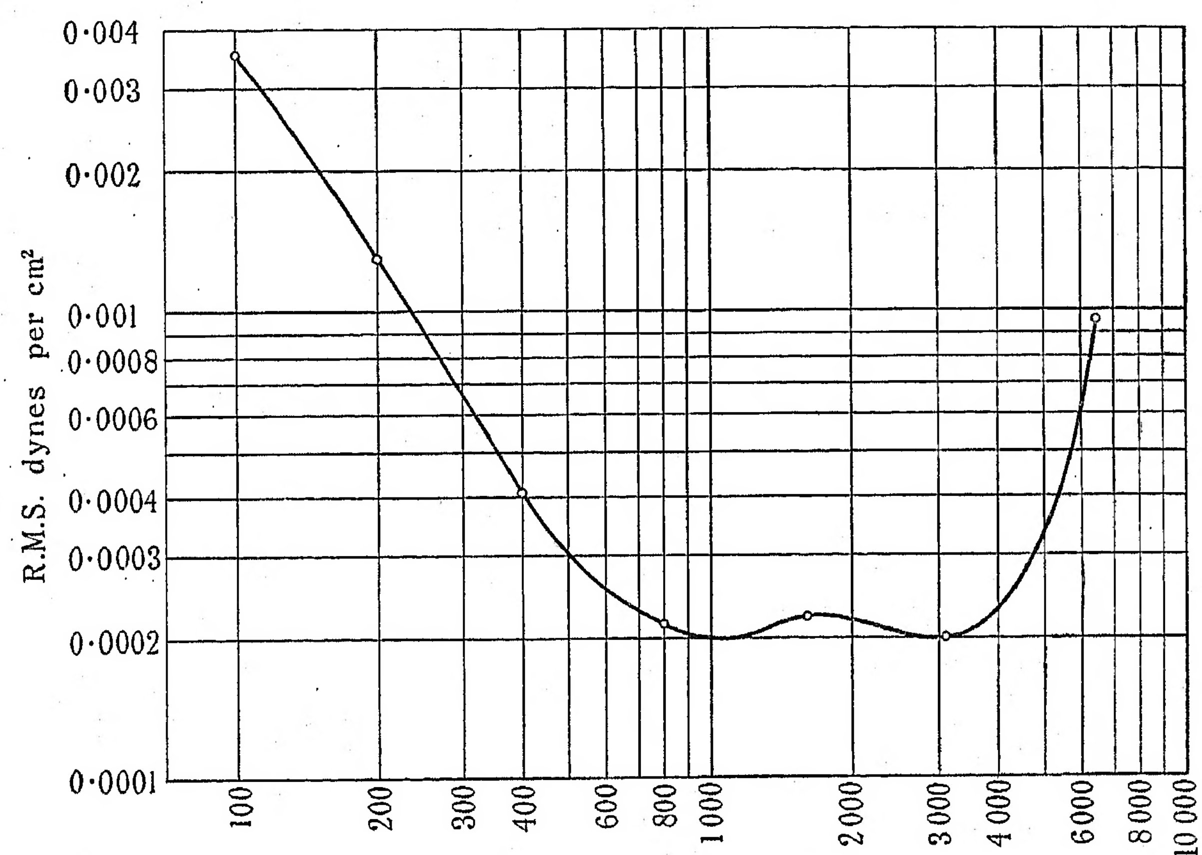


FIG. 1.—Threshold of hearing: field pressure for an observer facing the source.

In considering the results, the problem is to find at each frequency the most representative value of threshold intensity. It was felt, therefore, that the results should be presented for averaging on a scale proportional to sensation rather than stimulus. It is indicated in Section 2 (c) below (page 406) that, in the vicinity of threshold, sensation is proportional to decibels above threshold; so that the results, having been determined in decibels from certain defined intensities, were ready for any linear method of averaging.

Of the various available methods of averaging, it appeared that the most representative value of the

threshold is given by the mode of the results. The values obtained are given in Table 1, where they are specified in absolute units. In addition, the mean values on the decibel scale, or geometric mean of the pressures, and the mean and standard deviations, are given. The mean deviation is included because it is much less dependent on single erratic results than the r.m.s. value. Column 6 of Table 1 gives the decibels below 1 dyne per cm^2 of the modal values of column 2.

Of the 48 subjects tested, two groups, one of 15 males and the other of 14 females, were specially chosen so as to be age-graded, the ranges covered being 15–63 and 15–48 years respectively.

No marked change with age in either group could be noticed at 100 cycles per sec., but the female group is on the average 2 to 3 decibels less sensitive than the male. At 800 cycles per sec. there is a tendency in both groups for sensitivity to decrease with increasing age, and the female group is now 2 to 3 decibels more sensitive than the male. At 6 400 cycles per sec. the average sensitivities of the two groups are approximately equal, though the three oldest males have a much lower sensitivity than the rest. This is not the case in regard to the females, possibly owing to the more restricted age range.

It would be unwise to generalize as to the effects of age and sex on the threshold intensities from results on such small groups as 14 and 15 subjects. The most marked effect noticed was the reduction in sensitivity with age at high frequencies, but even then one of the youngest subjects tested, though reputedly normal, showed a low sensitivity at most frequencies.

In addition to the above determinations of the threshold field pressures with the subjects facing the source, a series of measurements was carried out on 10 individuals to determine the difference between sitting facing and sideways to the source. The difference is clearly bound up with the distortion of the sound field by the listener's head, and therefore depends on the dimensions of the head and exact location of the ear compared with the wavelength of the incident sound. It was therefore deemed sufficient to determine the effect in the case of 10 people of average size who are accustomed to making electrical measurements.

The dispositions of the source and subject were as in the previous threshold determinations, and the procedure was the same except for the additional measurements with the subject sitting sideways so that first one ear and then the other was turned towards the source. In each case an observer operated the controls. The measurements were made at the same frequencies as the previous threshold determinations, in order to ascertain the corrections necessary to make those determinations apply to sideways listening. The average differences between the two threshold field pressures for the 10 subjects tested are given in the last column of Table 1, the sideways condition being nearly always more sensitive than that of facing the source.

TABLE 1.

Field Threshold Pressures.

Frequency	Observer facing source					Increase in sensitivity due to turning head sideways
	Mode pressure	Mean pressure	Standard deviation	Mean deviation	Decibels below 1 dyne per cm^2. Mode	
(1)	(2)	(3)	(4)	(5)	(6)	(7)
cycles per sec.	dynes per cm^2	dynes per cm^2	decibels	decibels		decibels
100	0·00350	0·0034	4·7	3·8	49·1	0
200	0·00128	0·00138	4·5	3·3	57·9	0
400	0·00041	0·00050	4·0	3·2	67·8	0·5
800	0·000215	0·00028	4·9	4·0	73·4	2·0
1 600	0·000225	0·00045	6·3	5·2	72·8	0·5
3 200	0·00020	0·00035	9·8	6·5	74·0	1·0
6 400	0·00097	0·00186	9·6	6·6	60·3	6·0

(*b*) *The Relation between Decibels above Threshold for Equal Loudness at Different Frequencies.*

The only work on this subject known to the authors was that of Kingsbury,* who had made a direct comparison of the loudness of pure tones of from 60 to 4 000 cycles per sec. using a telephone as his source. Since his results were given in terms of the decibels above threshold at the respective frequencies, it was not anticipated that they would be affected by a change from telephone to free-space conditions of listening, as might have been the case if absolute units of pressure had been used. It was necessary, however, to extend the range of frequencies and amplitudes covered by Kingsbury, and the opportunity was therefore taken of examining as large a range as possible in a series of measurements corresponding to normal free-space listening. It was realized that it would not be possible to generate in free space pure tones of such large intensities as can be produced in a telephone, but it was hoped to demonstrate agreement between the two sets of results at low and medium intensities and so justify the use of telephones where necessary.

* B. A. KINGSBURY: "A Direct Comparison of the Loudness of Pure Tones," *Physical Review*, 1927, vol. 29, p. 588.

In the first series of tests, therefore, pure tones at octave intervals between 100 and 6 400 cycles per sec., generated and propagated in free space, were compared. To avoid comparing every tone with every other tone, each of the tones was compared with one of 800 cycles per sec. In addition to reducing the number of tests required, this procedure decreases the spread of the results, as the greater the ratio of the frequencies of two tones the more difficult and indefinite the comparison of their loudness becomes. Using the 800-cycle reference and (*b*). The sound intensities given by the telephone and loud-speaker were controlled by two attenuators adjustable in steps of 1 decibel. The procedure with each subject at each test frequency, therefore, was to determine first his threshold for the test tone from the loud-speaker and the reference tone in the telephone. The test tone was then increased in intensity in steps of 10 decibels and at each setting the decibels above threshold of the reference tone which appeared equally loud were noted. Readings were taken with the subject

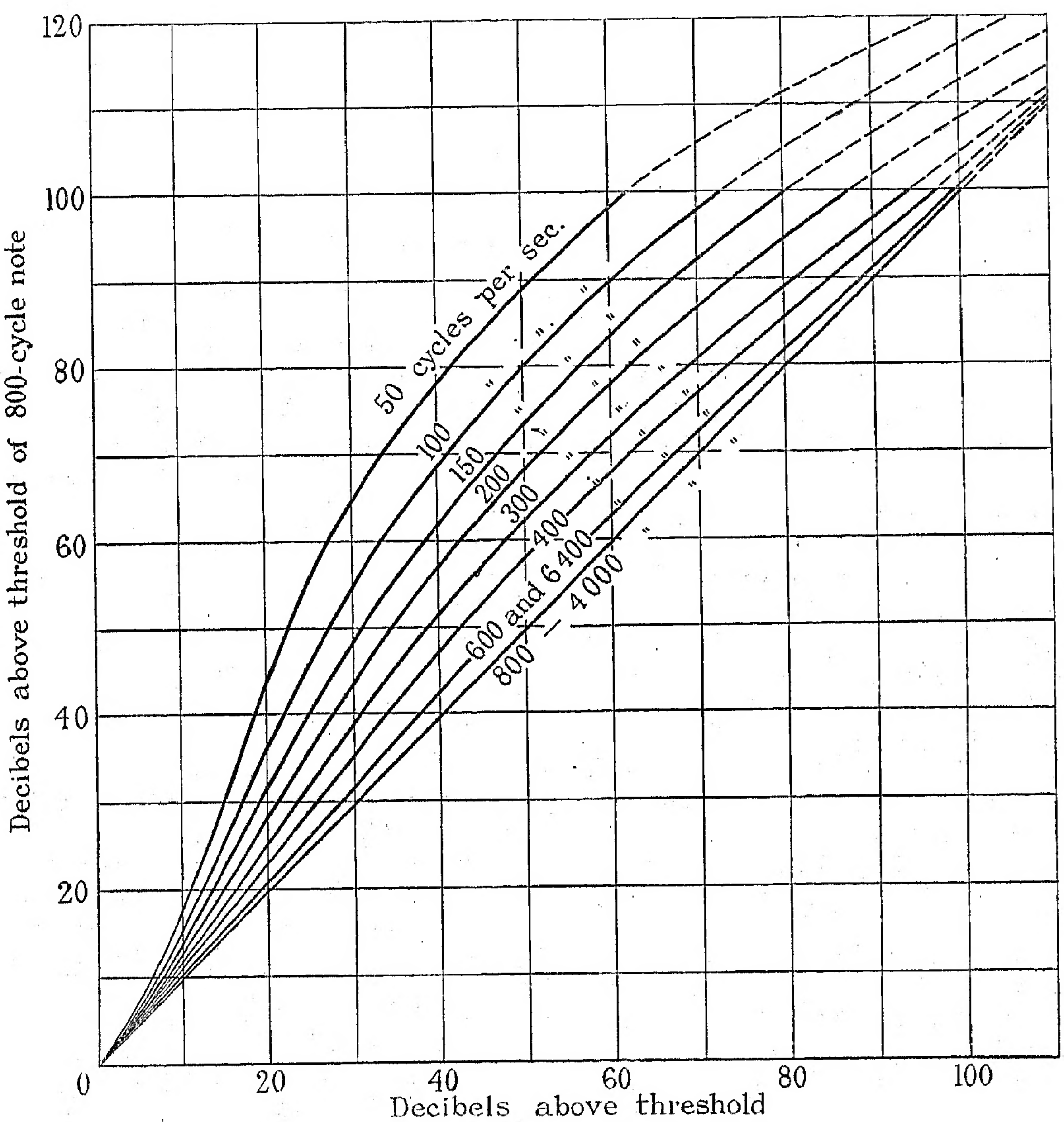


Fig. 2.—Decibels above threshold of equally loud notes.

tone, the subjects had little difficulty in determining the equal-loudness or balance condition. The legitimacy of the method was demonstrated by Kingsbury, who showed that if two tones were separately considered as loud as a third reference tone, they would appear equally loud when compared with each other. The point was also confirmed by the authors.

For convenience in making the measurements, the reference tone was presented to one of the observer's ears by a telephone while he sat on the chair in the non-reflecting room used for the threshold measurements so as to present his uncovered ear towards the loud-speaker source. This method of determining the intensity of a note which appears as loud as a given sound in free space is discussed again in more detail in Section 4, (*a*) seated the reverse way and with the functions of his two ears interchanged.

Since the measurements were intended only as a check on Kingsbury's figures, it was considered sufficient to carry out tests on 6 subjects who were known by the previous tests to be of average hearing ability. The range of intensity covered in this first series of tests was from threshold to 40 decibels at 100 cycles per sec., 80 decibels at 800 cycles per sec., and 50 decibels at 6 400 cycles per sec. Readings at higher intensities could not be obtained owing either to shortage of driving power for the loud-speaker or to the tendency to produce harmonics at large amplitudes.

The higher intensities were covered by the second series of tests using two telephones. The amplitude linearity

of the latter was checked by microphone measurements up to 110 decibels above threshold at 800 cycles per sec., and to corresponding intensities at other frequencies, so that the control attenuator readings gave a direct measure of the sound intensity in the observer's ears. Readings were taken on 6 subjects from threshold up to 100 decibels at 800 cycles per sec., so that the two series of tests had a large overlap.

In carrying out the measurements the subject was allowed to rest after each reading, and not more than 10 observations were made at one sitting. When measurements were repeated as a check, good consistency was obtained, showing that fatigue was not important.

In drawing the mean curves for each individual at each frequency of comparison it was clear that no distinction could be made between the two series of tests in the region where they overlapped. The results with two telephones were therefore accepted for the higher intensities as applying also to free-space conditions.

In order to compare the mean curves for all the subjects with Kingsbury's results, the latter were replotted at corresponding frequencies. This was done by taking the curves given in Fletcher's "Speech and Hearing"* and drawing the family of curves connecting the decibels above threshold of the equivalent 800-cycle note at typical intensities. A little smoothing was necessary in order to make these curves into a family. It was then a simple matter to draw the curves given in Fig. 2 for the same frequencies as had been used in these tests.

Comparing the results which had been obtained with the curves of Fig. 2, the 100-cycle results came slightly below and the 200-cycle results slightly above the Kingsbury curves. In both cases, however, the difference was less than the dispersion in the two sets of results. The 400-cycle results did not agree so well, but the 800-, 1 600-, and 3 200-cycle results agree very closely with the straight-line relation given by Kingsbury. The 6 400-cycle results agree with the curve for 600 cycles per sec.

In order to produce a family of curves as was done with the Kingsbury figures, the 400-cycle results required smoothing a maximum amount of 6 decibels. The agreement with the Kingsbury results of Fig. 2 was then satisfactory.

In view, therefore, of the greater number of subjects, namely 22, tested by Kingsbury, and the smallness of the difference between the two sets of results, it was decided to adopt his results redrawn as in Fig. 2 with the extension to 100 decibels and 6 400 cycles per sec.

(c) *The Relation Between Stimulus and Sensation.*

Both the old loudness scale used by the authors and the decibel scale are really logarithmic scales of physical stimulus. The justification, therefore, for their use to indicate subjective loudness has been the contention that such a logarithmic scale of stimulus corresponds to an arithmetic scale of sensation. This is another way of stating the Weber–Fechner law without any reservation as to the range of intensities over which it applies. Since the law was derived from experiments at medium intensities only, its application over the whole range from threshold to painfully loud intensities needs justifying. A much more insistent reason for suspecting the logarithmic relation between stimulus and sensation is provided by the common experience that the numbers assigned by such a scale to represent the sensations caused by various stimuli are not acceptable to introspection as a correct indication of their relative magnitudes. Experience shows that the rate of increase of loudness with the decibels above threshold is comparatively small at low intensities and much larger at high intensities. As a result the logarithmic stimulus/sensation scale appears very open at low, and cramped at high, intensities.

It was therefore decided to try to establish a definitely subjective scale of loudness by as direct a reference as possible to the judgment of the ear. In view of the work described in Section 4 (*b*), efforts were directed primarily towards establishing the relation between subjective loudness and the decibels above threshold for an 800-cycle note. The relations between stimuli in decibels above threshold at different frequencies for equal loudness established in Section 2 (*b*), however, make it a simple matter to transfer a scale at one frequency into the corresponding scale at another frequency.

An Integrated Loudness Scale.—If the loudness of a note be defined as the number of consecutive just-perceptible increments or decrements of intensity contained in the intensity range from threshold to that of the note considered, then, providing that the differential intensity sensitivity of the ear is known at all intensities in the audible range, an integrated stimulus/loudness scale can be readily constructed. On psychological grounds there are grave objections to the use of the just-perceptible changes as units of sensation, but it was nevertheless thought desirable to examine whether the form of the stimulus/sensation relation which would follow from this definition would prove acceptable to introspection.

Kingsbury (*loc. cit.*) had already constructed such a scale for a 1 000-cycle note using Knudsen's data* for the differential intensity sensitivity of the ear. Knudsen's work, however, was based on the sensitivity of the ear to cyclical changes of intensity. Such a procedure neglects the difference between the sensitivity to incremental and decremental changes which will necessarily be present since the ear is not linear with amplitude, and the subjective condition involved in testing with cyclical variations of intensity is entirely different from the condition involved when discrete changes are used.

It seemed desirable, therefore, to determine first the differential intensity sensitivity of the ear using discrete changes of intensity with the notes generated both in free space and in telephones applied to the ear. In addition, some measurements were made to determine the differential intensity sensitivity using cyclical changes. A detailed description of the work is being prepared for publication elsewhere, but an outline of the method and results may be of sufficient engineering interest to include here. The free-space measurements were carried out under the same conditions as the

* Published by Messrs. Macmillan, 1929.

* V. O. KNUDSEN: "The Sensibility of the Ear," *Physical Review*, 1923, vol. 21, p. 84.

threshold determinations, and, in addition, measurements were made on a number of subjects, using one and two telephones. Owing to difficulties caused by the transient response of the loud-speaker diaphragm, the free-space measurements were in doubt except at low intensities, where they agreed with the telephone measurements. The latter were taken up to 110 decibels above threshold with instantaneous changes and with durations of change of from $\frac{1}{4}$ to 3 sec. Over these ranges for isolated changes no difference could be found due to the speed of change or the use of one or two telephones. With cyclical changes there were distinct indications that the results were complicated by the retention of impressions by the hearing system, and therefore the results were not included in the final values given.

Disregarding the recurrent change tests, the results of several hundred observations on 4 subjects reduce to the curves of Fig. 3 for increasing and decreasing types of changes. It is not known, however, to what extent the results with decreasing intensities are affected by the retention of impressions, and hence the figures relating to increasing intensities only have been used in deducing the integrated curve given in Fig. 4.

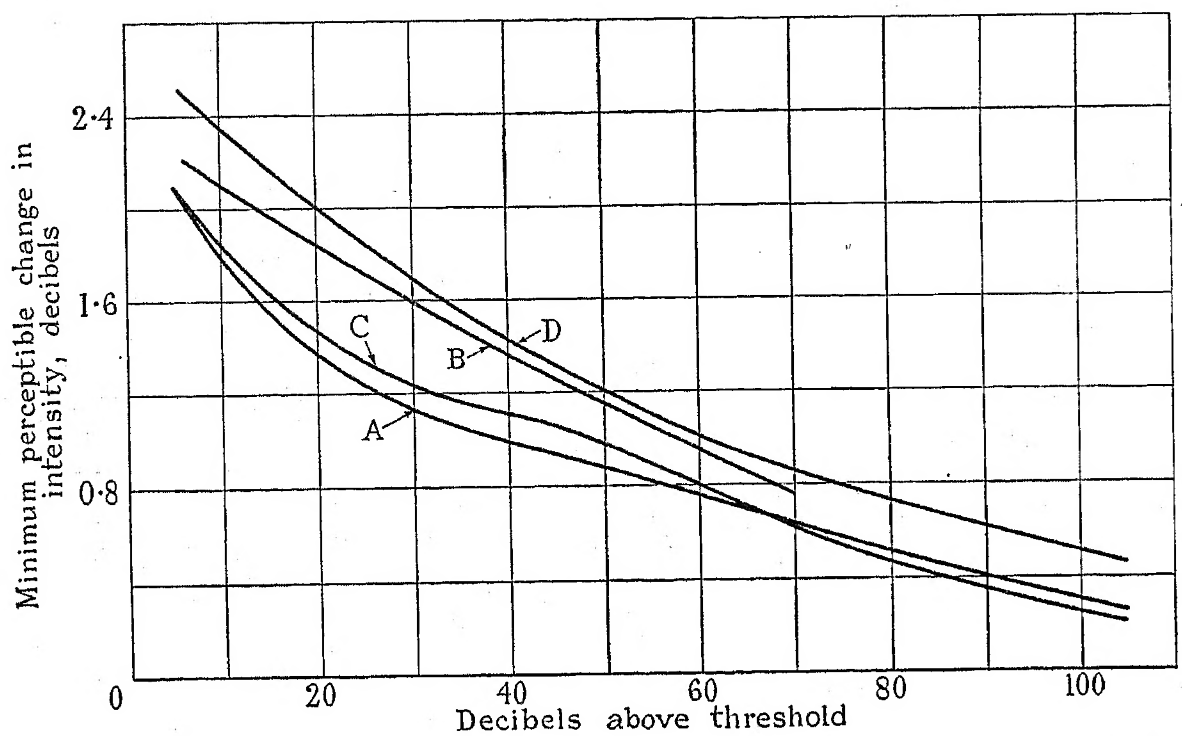


Fig. 3.—"Differential intensity sensitivity," determined with discrete intensity changes.

A. Incremental change by switching.
B. Decremental change by switching.
C. Incremental change by inductance variation.
D. Decremental change by inductance variation.

In considering this loudness scale, the authors' conclusion is that even if the assumptions on which it is based are accepted, the result, while an improvement on the decibel scale, still conflicts with mental estimates of loudness.

Multiple Loudness Scale.—A second and more direct method of determining the relation between loudness and stimulus intensity is by the method of mental estimates. This method had been employed over certain ranges by other investigators,* and it was desired to check and to extend their results. Initially attention was confined to an 800-cycle note, and one point on the scale was fixed by giving a loudness figure of 100 to the sensation resulting from an 800-cycle tone 100 decibels above threshold. The choice of number to attach to a given loudness level is unimportant, since it involves only the multiplication of the loudness-scale figures by a constant.

The procedure was to provide each of the subjects with a pair of telephones mounted on a headband, these receivers being fed, via a 2-way switch, from either of two attenuators supplied from the same 800-cycle source. The threshold intensity of the subject was first

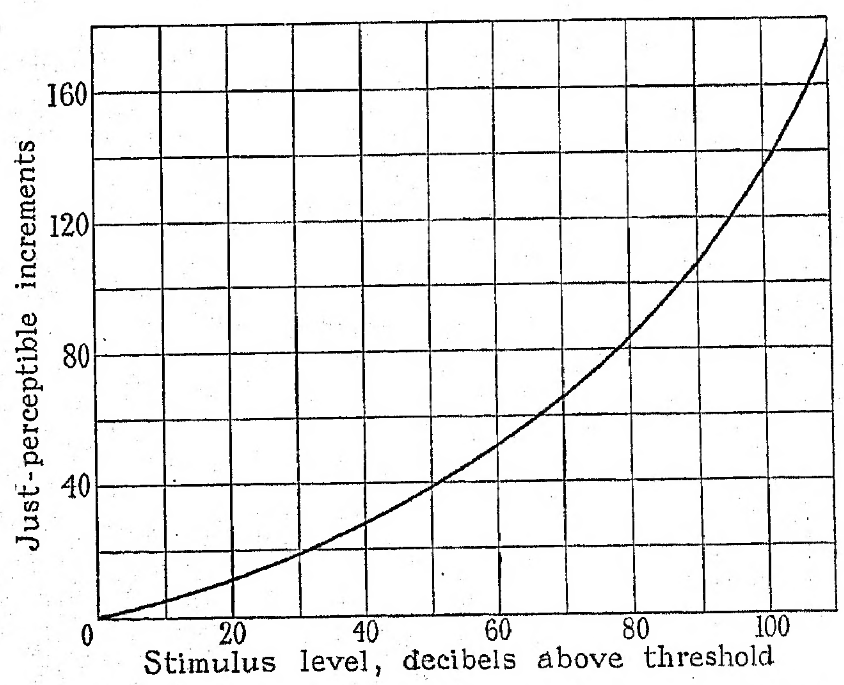


Fig. 4.—Relation between stimulus level and sum of just-perceptible increments.

* L. F. Richardson and — Ross: *Journal of General Psychology*, 1930, p. 288; D. A. Laird, E. Taylor, and H. H. Wille; *Journal of the Acoustical Society of America*, 1932, vol. 3, p. 393; L. B. Ham and J. S. Parkinson: *ibid.*, 1932, vol. 3, p. 511.

determined, and No. 1 attenuator was then set to give an intensity 100 decibels above threshold in the receivers. The subject was then asked to adjust No. 2 attenuator until moving the switch from position 1 to position 2 appeared to reduce the loudness of the tone to one-half. When this had been accomplished, No. 1 attenuator was given the same setting as No. 2, so that moving the switch produced no change of loudness, and the subject was required to readjust No. 2 attenuator until moving the switch from position 1 to position 2 again halved the loudness of the tone. This was repeated in all 6 times. If the loudness of the 100-decibel tone is designated 100, the subject thus determined the decibels above threshold required to give loudnesses of 50, 25,

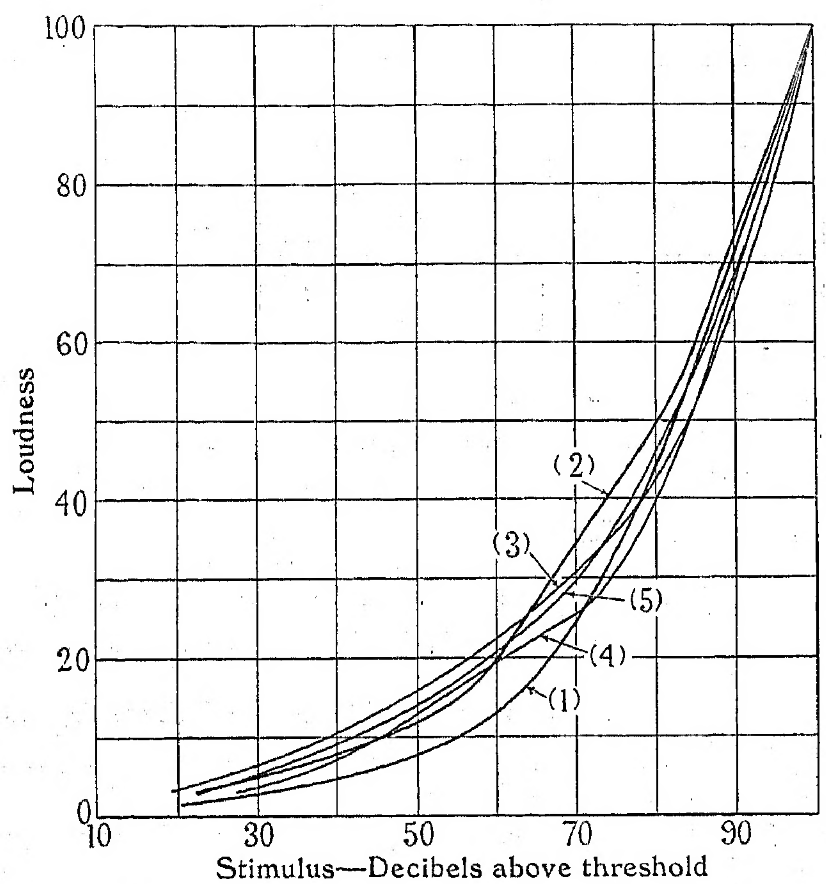


FIG. 5.—Individual consistency of estimated loudness-scale determination.

NOTE.—These curves show the result of 5 tests at 800 cycles per sec. on one person. Each curve was determined on a different day.
Tests (1) and (2) were made on consecutive days, and an interval of 6 months occurred before tests (3), (4), and (5) were made.

12·5, 6·25, 3·12, and 1·56 respectively. Care was taken throughout these tests to ensure a negligible background noise level, and the necessary precautions against fatigue were taken.

Initially some doubt was felt as to the practicability of determining what was meant by "half the loudness," and it was feared that subjects accustomed to sound measurement would tend to estimate rather the number of decibels above threshold than the loudness. Preliminary tests with 4 skilled observers showed that this tendency was not appreciable. Each determination of a loudness ratio appeared to be subjectively vague, but the consistency between repeated determinations made on different days was surprisingly high. As a check on the reality of the procedure, several runs were also made in which the subjects were required to adjust the notes to one-quarter of the loudness of the given note, instead of to one-half. This was done three times consecutively, and the loudness scale so determined was found to agree quite closely with that determined by consecutive halvings. This appears to establish that, in the case of loudness estimation, two consecutive estimates of halving do in fact agree substantially with an estimate of quartering. Fig. 6 shows the degree of consistency of one observer in estimating ratios of 2 : 1 and 4 : 1, and Fig. 5 shows the consistency of the same individual in making 5 repeat determinations of a series of 6 estimates of 2 : 1, these curves being taken at an interval of 6 months. It should be mentioned that during these tests the subjects did not observe the attenuator scale.

In view of the consistency of each of the 4 skilled subjects it was decided to apply the same method to a

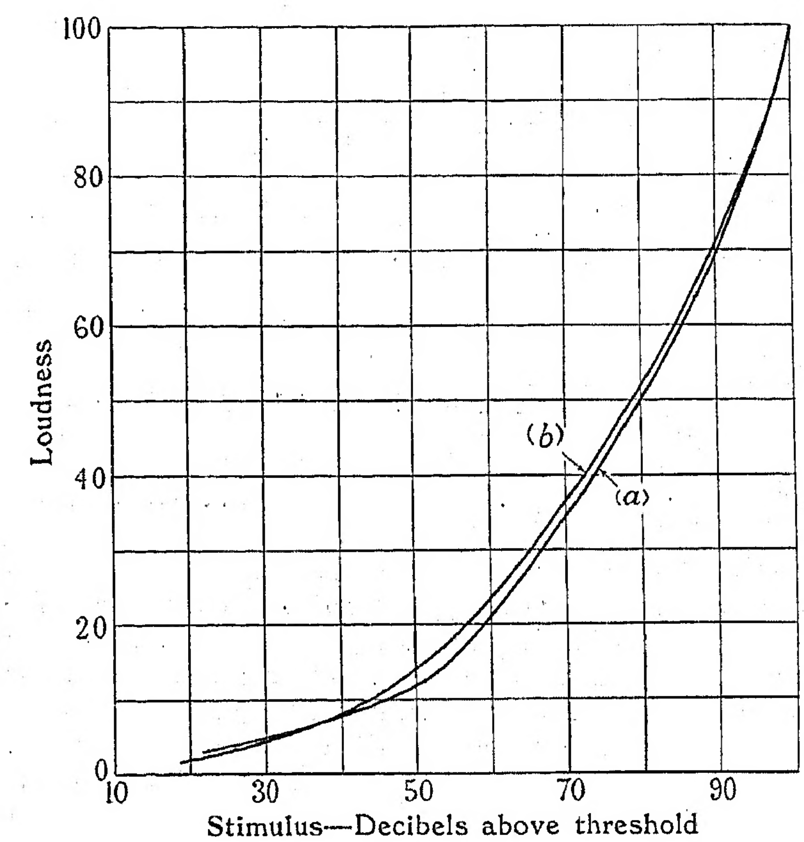


FIG. 6.—Estimated loudness scales by estimation of 2 : 1 and 4 : 1 ratios.

(a) Determined by estimating a loudness ratio of 2 : 1.
(b) Determined by estimating a loudness ratio of 4 : 1.
These two curves were determined on the same day by one person.
Curve (a) is curve (2) of Fig. 5.

group of 30 entirely unskilled subjects, 15 males and 15 females. The procedure with each of these was just as described above. It may be noted that few of these subjects appeared to experience any difficulty in deciding what constituted a loudness ratio of 2 : 1 or 4 : 1. A few expressed doubt initially as to their ability to determine such a ratio, but when it was attempted no great difficulty was apparently found.

In all, therefore, 34 subjects determined the intensities associated with the loudness figures of 50, 25, 12·5, 6·25, 3·12, and 1·56 respectively, by the estimation of loudness ratios of 2 : 1; and all but 4 of the males, that is 30 subjects, also determined the levels associated with the loudness figures of 25, 6·25, and 1·56 respectively, by the estimation of loudness ratios of 4 : 1.

Naturally the decibel figures obtained for any one loudness level show a considerable dispersion, increasing

at the lower levels. The procedure adopted has been to take the means of the decibel figures recorded at each loudness level. These give a series of points which fall on the smooth curves shown in Fig. 7. The figures, together with the standard deviations and the mean deviations, are given in Table 2.

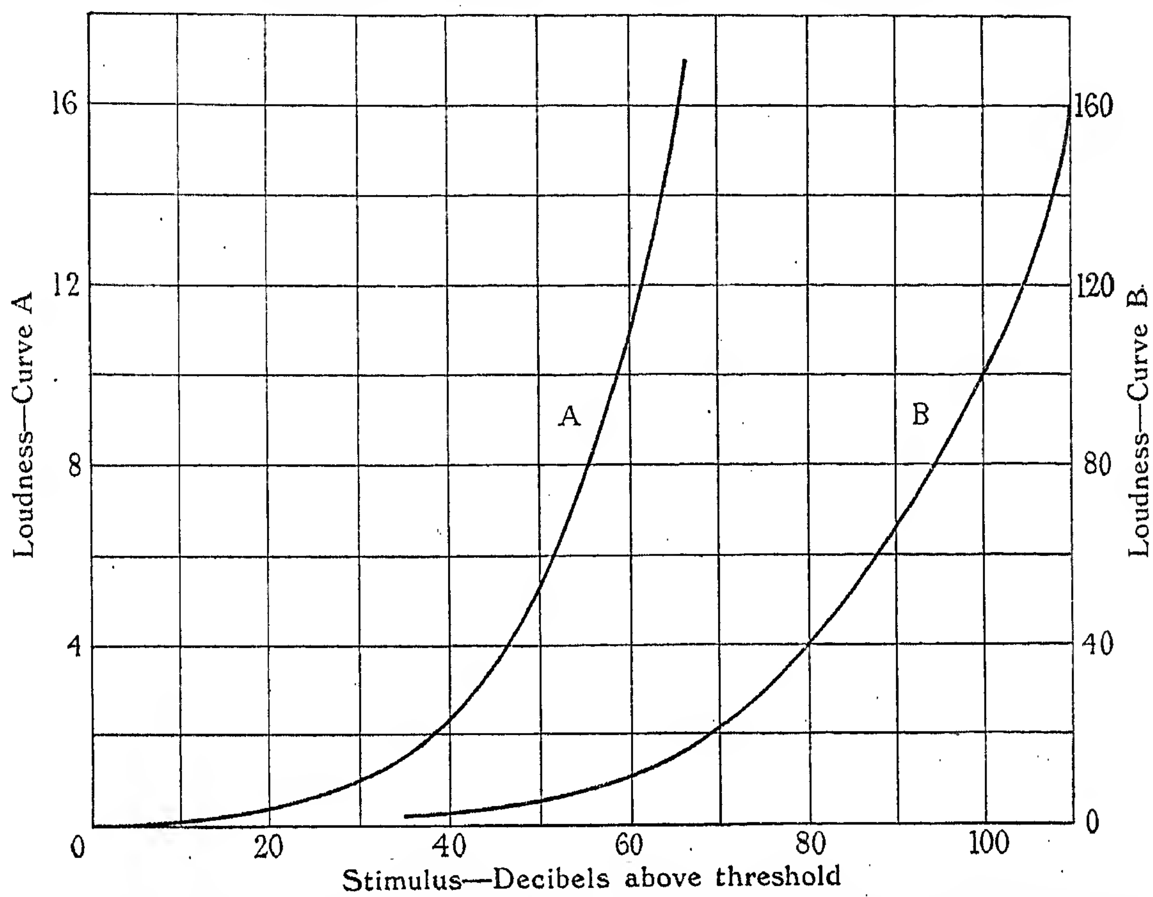


FIG. 7.—Relation between loudness and decibels above threshold for a pure 800-cycle note.
NOTE.—100 decibels are taken as equivalent to 100 loudness. Curves are determined from the fractional loudness estimates of 34 persons with normal hearing.

It will be observed that Fig. 7 is extended to higher and to lower intensities than those quoted above. It was found to be undesirable to present unskilled subjects with initial notes greater than 100 decibels, and difficulty was usually experienced at the very low levels. A number of tests was therefore made by 4 skilled subjects from levels of 110 down to 15 decibels, and these results, which agree well with the others over the range where they overlap, have been used to extend the curve over the complete range of 0 to 110 decibels as shown in Fig. 7.

Finally a series of tests was made with 3 skilled observers determining a loudness scale in a similar manner at different frequencies. The curves of Fig. 2 show that an 800-cycle tone 100 decibels above threshold has the same loudness as a 200-cycle tone 87 decibels above threshold. The initial 200-cycle note of 87 decibels was therefore called 100 loudness, and with the same procedure as before the decibels above threshold for other loudnesses were found. The result of such a test for 1 observer is shown in Fig. 8, curve 2. If each intensity of the 200-cycle tone on this curve is replaced by its equally loud 800-cycle tone (from Fig. 2), curve 4 of Fig. 8 results, and it will be seen that this agrees closely with the loudness scale determined directly at 800 cycles per sec. Tests were also made at 2 000 cycles per sec. (Fig. 8, curve 3). In this case a note represented by a given number of decibels above threshold is as loud as for an 800-cycle note, and no transposition is required. Fig. 8 shows the degree of consistency obtained by one skilled observer in making determinations at 200, 800, and 2 000 cycles per sec. All three observers showed good agreement.

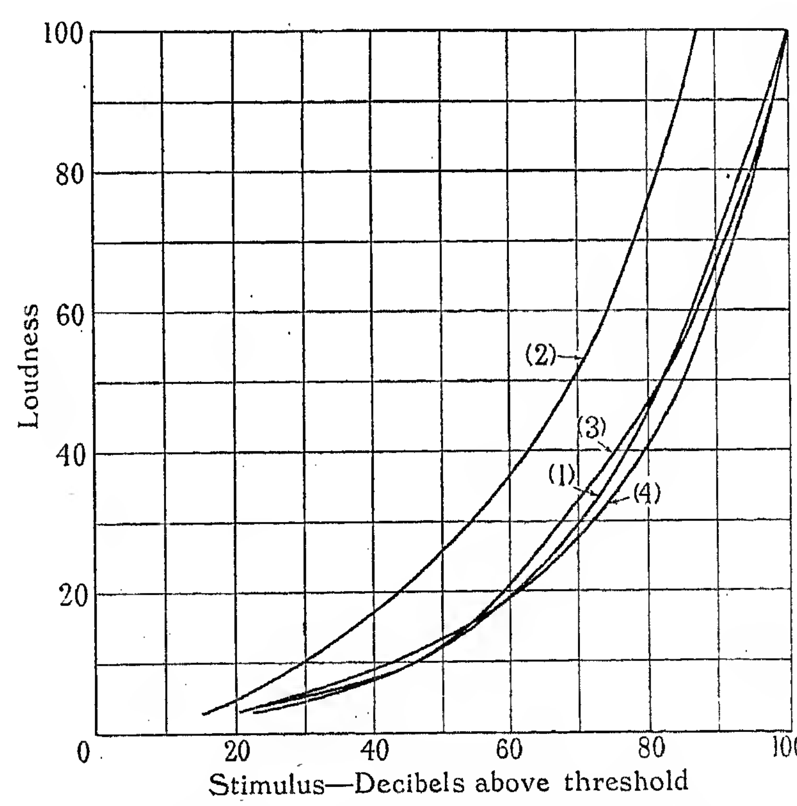


FIG. 8.—Estimated loudness scales at different frequencies.
All curves determined by one person estimating loudness ratio of 2 : 1.
(1) Determined at 800 cycles per sec. (mean of 5 tests on Fig. 5).
(2) Determined at 200 cycles per sec. (mean of 2 tests on different days, in close agreement).
(3) Determined at 2 000 cycles per sec.
(4) Curve (2) transferred into equivalent 800-cycle levels from Fig. 2.

Objections have been raised to the validity of mental estimates of sensations. These results, however, show that quite a high degree of consistency of determinations can be obtained by different methods. It appears certain, therefore, that some quantity is being measured, the only doubt remaining being whether that quantity is really a sensation. A comprehensive discussion of the psychological aspects of the problem would not be in place here; it is sufficient to say that if the quantity measured is not the loudness produced by the given stimulus it is certainly what the normal person means when he speaks of the loudness. Even if such an idea is unacceptable on formal psychological grounds, it is, nevertheless, the conception which the ordinary person forms and upon which he acts. It is therefore decidedly the quantity in which we are interested, since it at least determines the attitude of the ordinary person to industrial noises.

This scale has been used by the authors for all subsequent work, and has in all cases been found to give acceptable results.

TABLE 2A.

Estimation of Loudness Ratio 2 : 1 *at* 800 *cycles per sec.* (34 *subjects*).

Loudness figure	Stimulus level (mean)	Standard deviation of observations	Mean deviation of observations
	decibels	decibels	decibels
100	100	—	—
50	84	5·5	4·1
25	72	9·0	7·3
12·5	62	11·9	9·7
6·25	52	14·3	12·1
3·12	43·5	15·7	13·5
1·56	35·5	16·3	14·8

TABLE 2B.

Estimation of Loudness Ratio 4 : 1 *at* 800 *cycles per sec.* (30 *subjects*).

Loudness figure	Stimulus level (mean)	Standard deviation of observations	Mean deviation of observations
	decibels	decibels	decibels
100	100	—	—
25	70	8·7	6·6
6·25	54	11·6	9·2
1·56	40	13·8	11·8

(3) THE NOISE-ANALYSING APPARATUS.

(a) *Requirements Suggested by Experience.*

The apparatus described in the previous paper* was very useful in the early stages of the work, but as it progressed and the noises to be studied became less and less in magnitude, the sensitivity, even with an additional amplifier, was not sufficient for reliable measurements. The reason for this position became clear when the figures obtained by the authors for the average threshold were compared with those used as a basis for the design of that equipment. The greater sensitivity required called for a correspondingly greater range of control of sensitivity; of the order of 100 000 to 1. The selectivity of the apparatus had proved to be sufficient in the most difficult case encountered, namely a turbine gear with two important components 3·5 per cent different in frequency, and therefore no immediate improvement was necessary in this respect.

The arrangement of the apparatus to operate from an electric supply, while giving ample power without the inconvenience of batteries, introduced other troubles, particularly with high amplifications, arising from disturbances on the supply system. It also limited the sphere of usefulness of the apparatus to areas supplied with alternating current of suitable voltage and frequency.

As in some cases the time taken by an analysis is of primary importance, quicker operation without sacrificing other qualities demanded careful attention.

The sensitivity of the moving-coil microphone to stray magnetic fields proved to be a great drawback when testing certain pieces of electrical apparatus, and a change to a more generally applicable type therefore appeared to be desirable.

(b) *Apparatus to Meet these Requirements.*

In order to secure an apparatus possessing these desirable qualities it was necessary to make considerable changes in the detail design. The apparatus which has been evolved is, however, still of the same general scheme as that described in the previous paper. The microphone is of the condenser type, specially developed in a small size and with a plane front so as to cause a minimum of disturbance to the sound field being explored. It is supported on a stand so as to have a low natural period of vibration and is connected by 15 yards of flexible screened cable to the first amplifier. This is quite self-contained in a portable box with batteries and meters for checking the operating conditions of the valves. The output circuit of this amplifier is of very low impedance, so that a long screened cable can be used to connect it to the second amplifier, which is also self-contained and portable, and is placed in any convenient spot up to 30 yards from the first amplifier. The latter can therefore be near the machine being tested, say on a test-bed, while the second amplifier and portable frequency-analyser can be placed in an office where measurements can be made undisturbed. The arrangements for the control of the necessary enormous range of amplification have been evolved after considerable experiment. A total range of adjustment of amplification of 10^6, or 120 decibels, is provided in steps of 10 decibels.

Flexible screened cables connect the second box to the third, containing the frequency analyser and final meter. Although the analyser still consists of two loosely-coupled tuned circuits with the same degree of selectivity as before, the design has been so much improved that the instrument is now readily portable. In addition, the tuning is now continuously variable by a single calibrated dial over five overlapping ranges from

* *Loc. cit.*

20 to 25 000 cycles per sec. This is achieved by using accurately-matched variable inductances instead of three dial-type variable condensers. A great saving in time results from the speed with which the single tuning-dial can be turned when searching for components and also from the direct calibration of the dial in frequency. The only other tuning adjustment is that of the switch to change to the required range. The output meter, of the quick-reading copper-oxide rectifier type, is graduated in decibels, and this reading, added to the overall calibration constant corresponding to the frequency setting and the position of the attenuator, gives the sound intensity in decibels above threshold acting on the microphone. When an average reading of a rapidly varying component is to be taken, a small switch provides electrical damping for the output meter.

The overall amplification of the apparatus at the frequency corresponding to the analyser setting is of the order of 10^7, or 140 decibels.

(4) The Assessment of Total Noise.

(a) *Consideration of Some of the Available Methods of Assessment.*

While the problem of analysing a complex noise into its constituent tones and stating their magnitudes and frequencies presents some difficulties, yet the work involved is essentially in the realm of physics and the results are susceptible of incontrovertible statement in absolute units. Even when the intensity values are expressed in terms of the decibels above some generally-accepted values of the threshold intensities at the respective frequencies, the results are still unassailable. Quite other considerations arise when an attempt is made to assess the loudness of the noise as a whole. It has been shown in Section (2) how each of the components of the noise can be rated in terms of its equally loud 800-cycle tone, and that, using the loudness scale of Fig. 7, figures can be given for the loudness of each of the components heard separately. The loudness figures are no longer on an absolute basis and their summation into a figure to represent the total loudness of the noise as heard by the ear presents many difficulties.

The methods of determining the total loudness of a noise can be roughly divided into two classes, subjective and objective, the former involving the judgment of the human ear and the latter the interpretation of instrumental measurements in the light of the available data relating to the properties of the ear. The two classes can be further subdivided in the following way.

Dealing first with subjective methods, in the "equality" or "balance" method a reference tone which can be specified in frequency and intensity is adjusted so that it is judged by a representative person to sound as loud as the noise in question. The reference tone may be produced in free space but is more usually generated in either one or two telephone earpieces spaced off the head,* or in one telephone placed over one ear† while the other ear listens to the noise being measured. A tuning fork held to one ear has also been used. In the "masking" method a sound generated in telephones spaced off the ears, or by a tuning fork, is made to be either so loud as to just mask the noise in question or so soft as to be masked by it. The quantity derived in this case is not the magnitude of the noise but the deafening effect of one or other of the sounds.

Turning now to objective methods, where the total noise is to be assessed by calculation from analysis, the several components of a noise are determined by analysis and their intensities weighted according to their frequencies and the sensitivity of the ear at those frequencies. The weighted components would then be summed mathematically in a way to correspond to the action of the ear. No method of summation of general validity has yet been put forward. In the "artificial ear"* method, the weighting and summing of the components of a noise are attempted by electrical circuits and an indicating instrument.

The methods briefly indicated above were considered in detail with a view to determining which is the most trustworthy for quantitative measurements under engineering conditions. In the first place, comparing the subjective and objective classes of assessment, while we have in the former a direct reference to the final judge, the human ear, we introduce the personal factor into the observations, whereas in the latter the difficulty of simulating instrumentally or mathematically the response of the ear is offset by the much greater consistency of instrumental readings. The relative importance of these points must be considered along with the other features such as cost, convenience, and speed of working.

Objective Methods of Assessment.—Even supposing a method of summation by calculation were available, the calculations would necessarily be extremely laborious owing to the complexity of the characteristics of the ear. Moreover, fairly elaborate analysing equipment is required to obtain the initial data. Calculation from analysis is therefore never likely to become a method generally applicable to engineering problems, although it would undoubtedly be of value for research purposes.

The action of the artificial ear is presumably based on the following assumptions: (1) That the pure tone which, as in Section 2 (*b*), appears as loud as another pure tone by itself, still appears so when other frequencies, harmonic or otherwise, are present. (2) That the loudness of a complex noise is equal to that of a series of tones of a common frequency and severally equal in loudness to the components of the complex noise. (3) That the law of summation of such a collection of pure tones is known. It seems probable that the first assumption holds for the more important components of a noise, but it is not true for the small ones owing to selective masking. The second assumption is certainly untrue in many cases, since it neglects the subjective sum and difference tones which are formed by each component with every other component present. The third assumption introduces the question of the relative phasing of the pure-tone equivalents. The difficulty is avoided if the ear is responsive to the total energy of these equivalents. This is supported by the apparent independence of the ear to the phasing of the components of a steady sound, at least at moderate intensities. The above assumptions

* H. Fletcher: "Speech and Hearing."
† A. H. Davis: *Nature*, 1930, vol. 125, p. 48.

* The term "artificial ear" is used here to indicate an apparatus simulating the response of the ear. This is quite distinct from its use to indicate a device of similar acoustic impedance.

amount to taking the sum of the energies of the equivalent tones of common frequency and expressing it as a pure tone of that frequency.

In spite of the doubtful validity of the assumptions implied, this method of determining loudness has been used in a number of noise-measuring instruments. It was clearly desirable, therefore, to check whether the equivalent 800-cycle note calculated on this basis from the analysis of a noise does in fact agree with the observed equivalent note.

Table 3 shows the subjectively measured and the calculated equivalent 800-cycle notes of a number of noises representative of those encountered in engineering. The results are given in decibels above threshold, as it is unnecessary to convert to loudness for this purpose. For each source, the analyses on which these calculations are based and the direct measurements were made at the same point. In some cases this point was very close to the machine, and therefore these figures should not be taken as representative of the various sources.

It is seen from Table 3 that in all cases the calculated value is lower than the measured value. Apart from the assumptions already mentioned, this result may have been due to some extent to the presence of components outside the frequency range, namely 100 to 6 800 cycles per sec., of the analyser that was available, or to the difficulty of analysing and allowing for the "band" components. In these bands the energy is distributed fairly uniformly and, so far as can be ascertained with an analyser of finite resolving power, continuously, over certain frequency ranges. This effect is very much in evidence in connection with noises such as the hissing of steam or the rumble of heavy, slow-running machinery. It was hoped that these difficulties would be avoided by considering the noise of a transformer, as in that case all the components come within the range of the apparatus and are perfectly definite and discrete.

Table 3, however, shows that in the case of a transformer noise the agreement between the measured and the calculated levels was far less than in any of the other cases examined, and it was therefore necessary to consider in what respects the transformer noise had special distinguishing characteristics. Two such properties are obvious. In the first place, the transformer noise is lower-pitched, that is the loudest components are of lower frequencies than are the noises usually associated with most other types of machinery. In the second place, the transformer noise consists of a fundamental and a complete series of harmonics with no other components. In order to examine further the cause of the large discrepancy between the observed and calculated equivalent 800-cycle intensities and to ascertain which of the distinctive properties of a transformer noise caused this effect, a series of measurements was made on synthetic noises. Various notes of known constitution were reproduced in a loud-speaker mounted in the absorbently lined room previously mentioned, a condenser microphone being placed usually at a distance of 1 metre from the source. In each case the sound field was analysed, so that the frequency and the acoustic pressure of each component were known, and therefore the decibels above threshold and the equivalent 800-cycle note (Fig. 2) could be deduced and the summed-energy calculation made. Direct assessments of the noise were also made by 4 skilled observers using an 800-cycle reference tone and the technique described in Section 4 (*c*).

Notes consisting of two pure tones of equal loudness were first examined, and measurements were made with one component at twice the frequency of the other and also with the components not harmonically related. As will be seen from Table 5, the discrepancy was greater when the notes were harmonically related, but the discrepancies involved are not large enough to be conclusive. As the use of a multiplicity of separately-controlled tones introduces obvious difficulties, it was decided to use a multivibrator to produce a harmonic series of tones in

TABLE 3.

Complex Noises, Calculated from the Summed Equivalent Energy and Directly Measured Subjectively by Four Observers Using the Technique described in Section 4 (c).

Source	Number of components	Frequency range of components	Calculated equivalent 800-cycle note	Measured equivalent 800-cycle note	Difference, measured—calculated
		cycles per sec.	decibels above threshold	decibels above threshold	decibels
(1) 225-kW turbo-generator gear, 6 000/1 000 r.p.m.	20	100–5 700	90	91	1
(2) 3-phase induction motor, 120 h.p., 750 r.p.m., 50 cycles per sec., on load	12	120–3 400	82	89	7
(3) As No. (2), no load, coupled to loading machine	5	125–1 080	79	84	5
(4) As No. (2), no load, uncoupled	6	130–1 080	75	84	9
(5) Transformer, 110 kVA, 3-phase, 50 cycles per sec.	8	100–800	45	76	31
(6) Transformer, 150 kVA, 3-phase, 50 cycles per sec.	9	100–1 000	58	86	28
(7) Roller chain, 500 r.p.m.	8	375–2 480	65	85	20
(8) Roller chain, 1 000 r.p.m...	10	510–6 120	84	93	9
(9) 1 000-kW turbo-generator gear, 5 000/1 000 r.p.m.	23	100–2 700	97	107	10
(10) 250-h.p. fan, 90 r.p.m.	6	132–3 840 and bands	81	90	9

the loud-speaker. With a fundamental of 108 cycles per sec. a considerable discrepancy between the "summed equivalent energy" and the observed 800-cycle decibels was noted, and this was again found when the harmonic series had a fundamental of 500 cycles per sec. It appears, therefore, that the effect is one of the properties of a harmonic range of tones and is not peculiar to a low-pitched note. The reason for this is not known with certainty, but the authors have suggested* that it may be a consequence of the non-linear response of the ear, involving the production of subjective sum and difference tones which, in the case of a noise composed of a harmonic range of frequencies, would in many cases be directly additive to components objectively present.

This effect has been noticed in connection with reference tones of different frequencies, some of the results being shown in Table 4. Column (6) of this Table illustrates consistent disagreement between measured and calculated values due to the harmonic range of

TABLE 4.

Noise Measurement on a 150-kVA 50-cycle 3-phase Transformer: Mean of Four Observers.

(1) Cycles per sec.	(2) Decibels	(3) Decibels	(4) Decibels	(5) Decibels	(6) Decibels	(7) Decibels
100	34·5	51	61	81	20	− 5
200	41·5	65	59	82·5	23·5	− 3·5
400	50	79	58	85	27	− 1
800	58	86	58	86	28	—
1 600	58	87	58	87	29	+ 1
3 200	58	84	58	84	26	− 2

Column (1). Reference frequencies.
Column (2). Equivalent levels calculated from analysis for the frequencies of column (1).
Column (3). Levels directly measured with reference tones of the frequencies of column (1).
Column (4). Values in column (2) expressed in terms of equivalent 800-cycle levels.
Column (5). Values in column (3) expressed in terms of equivalent 800-cycle levels.
Column (6). Difference between measured and calculated 800-cycle values.
Column (7). Difference between measured 800-cycle and values of column (5).

frequencies existing in transformers, while column (7) shows that measured values depend to only a small extent on the frequency of the reference tone if the appropriate conversion is made.

Two other important effects may be noted. When each of the components of the 500-cycle range is reduced in intensity by roughly 40 decibels the summed equivalent-energy figure is also reduced by something like the same amount, actually from 83 to 48 decibels; in this case the observed equivalent intensity, however, dropped from 98 to 88, i.e. by only 10 decibels. This shows that the energy-summation method yields results which, in the case of a harmonic range, deviate more and more from observation as the intensity level of the noise is decreased, at least as far as levels of 40 decibels or so. It also illustrates the enormous reduction of radiated energy which is necessary in order to reduce to any appreciable extent the noise level of any apparatus such as a transformer which produces a harmonic series of tones. (A reduction of radiated energy in the ratio 10 000 : 1 in this case reduces the level only 10 decibels.) It also emphasizes the distinction between the energy of a sound and the energy of the equally loud 800-cycle note, which does not necessarily change in the same ratio. The former in this case changes in the ratio 10 000 : 1, whilst the latter only changes in the ratio 10 : 1.

* *Nature*, 1933, vol. 132, p. 350.

TABLE 5.

The "summed equivalent energy" 800-cycle note, and the equally loud 800-cycle note observed (using the technique of Section 4 (c), for various sounds.

Sound No.*	Loudest component frequency, cycles per sec.	Equivalent 800-cycle decibels: Largest component	Summed equivalent energy	Mean of observations by four observers	Difference (observed—summed energy)	
(1)	181/362	84	87	91	4	
(2)	181/362	80	82	89	7	
(3)	108	92	92	102	10	Harmonic relation
(4)	500	80	83	98	15	
(5)	500	45	48	88	40	
(6)	178/356	54	56	68	12	
(7)	147/360	83	87	89	2	
(8)	147/360	79	80	86	6	
(9)	108	92	92	90	−2	Non-harmonic relation
(10)	147/362	62	64	68	4	
(11)	180/316	55	58	66	8	

* The sounds designated by the numbers (1) to (11) were of the following composition:—

(1) Equally loud tones of 181 and 362 cycles per sec. respectively, and four small harmonics.
(2) As (1), but at lower intensity.
(3) Harmonic range; fundamental 108 cycles per sec.; 31 components; predominating fundamental.
(4) Harmonic range; fundamental 500 cycles per sec.; 12 components.
(5) As (4), but at lower intensity.
(6) Two tones, of 178 and 356 cycles per sec. respectively, having approximately the same loudness.
(7) Equally loud tones of 147 and 360 cycles per sec. respectively, and eight small harmonics.
(8) As (7), but at lower intensity.
(9) Pure tone, 108 cycles per sec.
(10) Two tones, of 147 and 362 cycles per sec. respectively, approximately equal in loudness but of lower intensity than (7) or (8).
(11) Equally loud tones of 180 and 316 cycles per sec. respectively.

A further point of considerable interest may be noted. In the case of the harmonic series with a fundamental of 108 cycles per sec. (Table 5) the fundamental was so large that the energy of the equivalent 800-cycle tones of all the other components was negligible, and the "summed energy" of the series was the same as that of the fundamental alone, i.e. 92 decibels, which was 10 decibels below the observed value (102). Using the fundamental alone on the loud-speaker, the observed level was now 90 decibels, representing a change of 12 decibels caused by the removal of components which did not appreciably contribute to the energy summation.

It appears, therefore, that, in a harmonic series at least, the small components do contribute to the total loudness much more than would be expected if the summation method outlined were correct, and it therefore follows that one or more of the assumptions made are not valid. The fact that the discrepancy increases as the level falls, indicates that some other function of the energy should be summed, but this cannot correct the differences in the discrepancies which occur when harmonic and non-harmonic ranges are considered. The method appears to give reasonably accurate results when applied to notes having a random frequency distribution of components at a high level, but gives considerable errors when applied to a harmonic range, these errors being very great indeed when the level of such a range is low. Since some harmonics are found in practically all noises, some error must always be present. The method is clearly not suitable for assessing all types of noises, as it would very unfairly discriminate in favour of harmonic-range types of noise, giving always a fictitiously low result when applied to transformer noises.

It is obvious that the same criticism must apply to the "artificial ear," because this device merely substitutes for the analysis of the noise and interpretation of the components by the equal-loudness contours, a filter system designed to weight the components in the same way. The subsequent addition of the weighted components on an energy basis is effected by an r.m.s. type of meter, so that if the weighting is correct the artificial ear should give immediately the same result as the analysis and calculation. In addition to the difficulty discussed above, however, the artificial ear is still further handicapped on account of the non-parallelism of the equal-loudness contours. This means that the weighting network must be adjusted for the range of intensity under consideration and that components out of that range will be weighted incorrectly. The latter point is not usually important, however, as the loudness of a complex noise is generally largely determined by the loudest components, those which are 10 or more decibels smaller contributing little unless present in large numbers.

An "artificial ear" constructed by the authors was found to confirm the above conclusions. Large errors were observed with harmonic ranges, especially at low intensities.

Since for engineering purposes it is clearly essential that the method of measurement chosen should be applicable to the hum of electrical machines and apparatus, the authors decided to discontinue the investigation of the objective methods of assessing loudness and concentrate on more promising methods.

Subjective Methods.—It is clear from the brief description given above that balance and masking criteria involve quite different aspects of the noise, the former yielding the magnitude of the noise and the latter its deafening effect upon another noise. The only justification for using the masking criterion as a measure of loudness would therefore be that there is a readily ascertainable relation between the two results. Fletcher* has shown that the relation for pure tones is complex and varies over a very wide range. It is therefore not to be expected that there will be, in general, any simple relation in the case of complex noises. For example, two noises of different character but both assessed at 70 decibels above threshold by an 800-cycle reference tone may just mask 800-cycle notes of 50 and 40 decibels respectively. Another objection to the use of the masking method for assessing loudness is encountered when considering quiet sounds of less than 40 decibels above threshold, because in such cases the masking effect is very small and is much more dependent on the threshold of the observer than the balance method. As a method of noise assessment of general application, therefore, the balance method is far preferable, the masking technique being much better reserved for its legitimate sphere of determining the maximum sound which is inaudible in a given background noise.

* "Speech and Hearing."

Once the method of noise assessment in terms of the standard sound which appears equally loud had been decided upon, it remained to specify the character of the standard and how to apply it, together with the noise, to the observer's ears.

There are several reasons for choosing as the standard a pure tone rather than a complex one. First, a simple standard is inherently desirable. A pure tone best fulfils this requirement and is capable of precise specification and reproduction. It has a known relation to pure tones of other frequencies, and shows the simplest relation between stimulus and sensation magnitudes. For these reasons it has become largely accepted as a standard of reference, whether it is used for actual noise observations or not. The use of a complex reference sound for observations has been advocated because of the supposed difficulty of balancing a pure tone against a complex noise. It will be shown later that the difficulty is more apparent than real. Apart from this, a complex reference sound, to be advantageous for engineering purposes, would have to have components extending down to 100 cycles per sec. or lower. This would then involve difficulties due to changes in relative loudness of the components at different intensity levels.

Further, it has already been shown that when the complex tone consists of a harmonic range of components, such as is obtained from some type of current interruptor and a telephone, the relation between the loudness of the note and that of the equivalent pure tone cannot be predicted. It was therefore decided to use a pure tone of specified frequency both as the reference standard and also as the working standard.

Davis* in this country had used an 800-cycle tone, whereas other investigators in America had used a frequency of 1 000 cycles per sec. The preliminary work with the method showed that some persons had difficulty in comparing the loudness of tones differing by more than 8 to 1 in frequency. The range of frequencies most commonly met with in engineering work extends from 100 to approximately 6 000 cycles per sec., so that, by choosing 800 cycles per sec., the ratio 8 : 1 is not normally exceeded.

It is shown in Section 2 (*b*) that notes of 800 to 4 000 cycles per sec. of the same number of decibels above their respective thresholds sound equally loud, so that it could not be objected that measurements in terms of an 800-cycle tone would not be directly comparable with others using 1 000 cycles per sec. and similar technique.

* *Nature*, 1930, vol. 125, p. 48.

A further important point which will arise later in considering the various methods of listening to the standard sound and to the noise simultaneously is that 800 cycles per sec. is the lowest frequency which is definitely free from the phenomenon of subjective beats.* There is also the advantage that in the region of 800 cycles per sec. the range of audible intensities is a maximum. It has been suggested that over the range 600 to 1 000 cycles per sec. there is an optimum frequency. The spread of a number of observations carried out by the authors at 640, 800, and 1 000 cycles per sec. on typical engineering noises showed only a slight reduction of spread for 800 cycles per sec. This may be merely a consequence of greater familiarity with this reference frequency than with the others. In view of the fact that 800 cycles per sec. has long been the standard frequency for communication engineering it was decided to adopt that frequency for the standard reference tone.

The problem of determining the intensity of the 800-cycle note which sounds as loud as a given noise raises several important questions, both physical and psychological.

At the outset it must be realized that loudness is a sensation, not a physical quantity, and that it has no existence outside the mind of the listener. The procedure is to find the physical magnitude of the standard stimulus which produces a loudness sensation of the same magnitude as that due to the source under observation. This judgment of loudness equality is an altogether simpler matter than the estimation of absolute loudness magnitude. It has been suggested that the term "awareness" is more appropriate than loudness, as the experimental procedure is, in fact, to adjust the standard stimulus in relation to the noise under observation until the observer is not more aware of one than the other.

If the noise can be started and stopped quickly there is the possibility of listening to it and the standard tone alternately. If each is heard long enough for a stable impression of its loudness to be formed, the only question involved is the capability of the observer to retain the impression of one long enough to compare with the other. The point is presumably covered by repeating the comparison in the reverse order a number of times.

There are two simple ways of producing the reference tone, one in free space by a loud-speaker and the other by telephone receivers. With the alternate listening method in non-reflecting surroundings the conditions in the two cases are most alike if a loud-speaker is arranged near the source of noise, with provision for a quick change-over from one to the other. The necessary equipment is very much simplified if, instead of the loud-speaker, a pair of telephones which can be quickly adjusted over the ears is used.

In practice it is not possible to start and stop the noise under consideration quickly enough for the alternate listening method, so that it is necessary to investigate the effect of listening to the noise and the reference tone simultaneously. If this is done by listening to both sounds with both ears, the character of the noise heard by the operator is different from what he normally hears in the absence of the reference tone. From the psychologist's point of view, the experience of listening to the two sounds together is a distinct, and generally different, experience from that of listening to them separately. There is in addition the difficulty of picking out the reference tone and comparing it with the sum of the components of the other noise. In order to hear the noise and the reference standard with both ears, a loud-speaker is the obvious, but inconvenient, way, a simpler way being to use a pair of telephones supported a little way from the operator's ears. The spaced telephone equipment, which has been used principally in America, has the disadvantage that it has a frequency-discriminating effect on the noise under consideration, the baffling effect of the telephone receivers reducing the high notes as compared with the low ones. This effect is illustrated by Table 6.

* C. E. Lane: *Physical Review*, 1925, vol. 26, p. 401.

This Table shows that with a plane wave the effect is large only at high frequencies, but tests on a motor and on a transformer in a reflecting room giving random incidence show considerably larger effects. This phenomenon is quite marked when listening to a noise containing high-frequency components, if the spaced telephones are quickly adjusted on the head. It follows, therefore, that such a noise would, apart from other considerations, be assessed lower than it should be.

TABLE 6.

Mean Results of Two Observers Facing Source in Non-Reflecting Room.

Frequency	Threshold shift with receiver spaced $\frac{7}{16}$ in.
cycles per sec.	decibels
400	0
800	0
1 600	2
3 200	4·5
6 400	6
12 800	9·5

Further, to produce by spaced telephones reference-tone intensities up to the 110 decibels required for engineering purposes needs an inordinately large acoustical output from the telephone (of the order of 20 decibels more, or 100 times the power) as compared with a telephone close to the head. The authors do not know of any telephone of ordinary construction which would preserve linearity under spaced conditions up to such an intensity.

The alternative method of listening simultaneously to the noise and the reference tone is to listen to one with one ear and the other with the other ear. This is effected by presenting a telephone earpiece to one ear and supplying it from a suitable source of 800-cycle current. A soft rubber cap on the telephone makes an effective seal, so that the amount of noise other than the reference tone heard by that ear is negligible (as shown in Table 7). The Table shows the insulating effect of a suitably chosen combination of rubber cap and telephone, the effect being determined by noting the change in threshold intensity due to wearing such a telephone and cap on each ear. The rubber cap also prevents appreciable conduction of vibration from the receiver to the bones of

the head. Using a telephone over one ear, the difficulty of picking out the reference tone disappears and, when it has been adjusted to loudness equality with the noise, both ears receive the same level of sound as they would when listening normally to the noise. It is felt, therefore, that in this method the disturbance from normal listening conditions is a minimum. In addition, the formation of subjective beats and of objective beats due to bone conduction* through the head is avoided. It is usually easier to make the balance adjustment when standing sideways to the source so that the noise and reference tone appear to be on opposite sides of the observer. This is particularly helpful when there is appreciable background noise present, but it must be remembered that, in general, the quality and intensity of a noise heard sideways is rather different from when heard facing the source.

TABLE 7.

Mean Results of Four Observers.

Frequency	Threshold shift
cycles per sec.	decibels
100	11
200	10
400	11
800	22
1 600	15
3 200	37
6 400	29

* H. FLETCHER: "Speech and Hearing," p. 193.

(*b*) *Comparison of the Subjective Balance Methods.*

In order to ascertain to what extent the above considerations affect the assessment of the loudness of a noise in practice, tests by all the above subjective balance methods have been carried out on various typical engineering noises. The examples chosen are representative of a wide range of intensity in the noise of electric motors and transformers. The values given are the means of the results of 4 skilled observers.

For convenience the methods are designated by letters as follows: (A) One ear towards noise, other ear covered by telephone; noise and reference tone heard simultaneously. (B) Same as (A), but heard alternately. (C) Reference tone in space; observer facing loud-speaker and noise; heard simultaneously. (D) Same as (C), but heard alternately. (E) Reference tone in space; observer sideways to loud-speaker; remoter ear covered; heard simultaneously. (F) Same as (E), but heard alternately. The check consisted of assessing by method A the loud-speaker intensity determined in E or F.

From the considerations discussed above it might be expected that methods A, B, and D, should agree, that methods C and E might show an effect due to the hearing of the noise and the reference tone simultaneously, and that E and F might show an effect due to the difference between the noise heard facing or sideways to the source. It might also be expected that the measurements using alternate listening to the noise and the reference tone, i.e. B, D, F, would show a greater spread of results than the others owing to the fading of impressions.

Reference to Table 8 suggests that, in the cases chosen, these expected tendencies, if present, are concealed by errors of observation. Although the greatest spread in results does occur in method D, it is not so in every case.

TABLE 8.

Agreement Between Various Methods of Subjective Balance. Mean of Four Observers.†

Method	(A)	(B)	(D)	(F)	Check (F)	Differences (A)–(B)	(A)–(D)	(A)–(F)	(A)–check
Motor noise	81	80	85	—	—	1	− 4	—	—
Spread	2	3	11	—	—	—	—	—	—
Motor noise	58	58	57	57	58	0	1	1	0
Spread	4	4	8	7	5	—	—	—	—

Method	(A)	(B)	(C)	(D)	(E)	(F)	Check (E)	Differences (A)–(B)	(A)–(C)	(A)–(D)	(A)–(E)	(A)–(F)
Transformer noise	86	84	91	80	85	84	84	2	− 5	6	1	2
Spread	2	2	2	1	4	3	1	—	—	—	—	—
Transformer noise	45	45	41	40	44	42	47	0	4	5	1	3
Spread	4	4	4	4	3	5	3	—	—	—	—	—

† The values refer to the equivalent 800-cycle intensity in decibels above threshold.

It appears, therefore, that in dealing with the complex noises usually encountered in engineering, the technique of balancing the reference tone against the noise is not critical.

It might still be argued that the choice of an 800-cycle reference tone made the balance method arbitrary, in that different results might be obtained with other reference-tone frequencies. As explained in Section 2 (*b*), Kingsbury had demonstrated that two pure tones which were separately assessed as being equal in loudness to a third pure tone were also equal to each other. It remained, therefore, to demonstrate that two pure tones which had been separately adjusted to equality in loudness with a complex noise were equal to each other. Measurements were made on the noise of a shaft running in ball bearings at 1 000 r.p.m. Method A was used with reference tones of 100, 400, 800, and 1 600 cycles per sec. Table 9 gives the values obtained in decibels above threshold at the respective frequencies, and also their 800-cycle equivalents from Fig. 2.

The close agreement between the direct and indirect assessments shown in the Table confirms that the result

TABLE 9.

*Agreement Using Different Reference Tones in Method A. Measurements made on the Noise of Ball Bearings.**

Reference-tone frequency	Assessment	Values converted to 800-cycle basis	Difference from direct 800-cycle assessment
cycles per sec.	decibels	decibels	decibels
100	67	95	3
400	86	91	− 1
800	92	92	—
1 600	89	89	− 3

* Assessments given in terms of the thresholds at the respective reference-tone frequencies. Means of four skilled observers.

is independent of the particular reference frequency used, provided that the figures obtained are first translated into terms of a standard frequency. Additional evidence on this point is afforded by Table 4, col. 7, where the discrepancies observed when measuring the noise of a transformer with different reference frequencies are given.

(*c*) *Details of the Method Chosen.*

In view of the results obtained with the various modifications of the subjective balance method the choice of a method suitable for use in engineering is a simple matter. The alternate listening methods B, D, and F, are precluded by the impossibility, in general, of stopping the source suddenly. Methods C and E, using a loud-speaker, would be most inconvenient and expensive, as a very large loud-speaker and power-supply unit would be necessary. The method involving the use of spaced telephones is ruled out on the grounds of the practical difficulty of applying it to large intensities and of its effect in attenuating the high-frequency components of a noise. We are therefore left with method A, using a single telephone. This method has therefore been adopted in all the subsequent noise measurements made by the authors.

The power required by a telephone to give an 800-cycle note of 110 decibels above threshold is of the order of 0·001 watt. The possibility of generating this by a buzzer and filtering out the harmonics was considered, but as constancy of frequency, amplitude, and purity, were of first importance, it was decided to use a small valve oscillator. A suitable arrangement was developed which gave sufficient output to feed a control attenuator and telephone with a d.c. input of only 12 volts, 5 mA. By suitable design the harmonic content of the output of this oscillator was made less than 2 per cent. The control attenuator for adjusting the intensity of the reference tone consists of a resistance network and tapping switch. The attenuator and oscillator are combined in one box, and two tapping switches are provided, one reading from 0 to 100 decibels in steps of 5 decibels and the other covering a range of + 6 to − 5 decibels continuously. This arrangement proves to be very convenient, as a smooth variation on each side of a roughly determined balance is particularly helpful in judging equality.

The most important requirement of the telephone is that it should be linear in its response up to the largest amplitudes used. The acoustic pressure generated in the telephone is then directly proportional to the current input, and there is no harmonic generation. In calibrating the telephone it must be remembered that the performance when it is held to the ear is quite different from that obtained when it is not. The calibration is effected by embedding in the rubber ear-cap a small copper tube connected to a microphone, and so measuring the acoustic pressure applied to the ear directly.

In order to relate the threshold pressure under these conditions to the free-space value previously determined for facing conditions, a series of measurements was made using a similar calibrated probe and microphone to determine on a number of persons for an 800-cycle note in free space the relation between the pressure in the opening of the ear and the field pressure in the absence of the subject. For 10 subjects the difference varied between 2 and 4 decibels with an average of 3 decibels, the pressure in the opening of the ear being always higher than the field pressure. The pressure in the opening of the ear which is taken as the threshold for the 800-cycle tone is therefore 3 decibels higher than 0·000215 dyne per cm^2, i.e. is 0·00030 dyne per cm^2. It is therefore a simple matter to determine the voltage at 800 cycles per sec. which must be applied to the attenuator in order to make it read directly in decibels above this average threshold. Measurements on 10 persons have shown that, with a given input to the telephone, variations in the generated pressure due to differences in the acoustical impedance of the ears do not exceed 0·5 decibel. Further measurements to check the reproducibility of the generated pressure with a given person showed that no variations larger than 0·1 decibel arose.

The selection and adjustment of telephones is a highly important matter if the calibration is to be permanent. The consistency of the telephones used is examined, and the most important effect, amounting to a change in

sensitivity up to 1 decibel, was traced to the warming of the diaphragm and clamping ring after being held on the ear for a few minutes. This increase in temperature presumably alters the elastic properties of the diaphragm and the degree of clamping at the edges, but as the effect does not amount to more than 1 decibel after 5 minutes on the ear the matter is not serious. As a result of the precautions taken, the permanence of calibration over long periods is satisfactory. A rectifier-type voltmeter is provided in the apparatus for setting the voltage applied to the attenuator to the calibration value. Adjustment is made in coarse steps by tappings on the H.T. battery, and continuous control is given by the filament rheostat. No great precision in setting the voltage is required, since it would need an error of 12 per cent in voltage to cause an error of 1 decibel. Since it is on comparatively rare occasions that noises in excess of 105 decibels have to be measured, the apparatus was made direct-reading up to 106 decibels. The linearity of the telephones used has, however, been checked in the way described up to 112 decibels. Readings up to this value can therefore be taken by adjusting the oscillator to give a higher voltage than the standard value and adding a corresponding number of decibels to the indications, e.g. doubling the voltage involves adding 6 decibels to the readings. The oscillator is then called upon to give the increased output only on rare occasions, thus effecting economies in battery consumption. The provision of a voltmeter enables the instrument immediately to be made direct-reading in terms of any reference pressure, which may subsequently be standardized for national or international use.

The complete apparatus is contained in a box 14 in. × $7\frac{1}{2}$ in. × $6\frac{3}{4}$ in. weighing 16 lb., and can be carried by a strap over the shoulders while a series of measurements is being made from point to point. In this position, and even when the head is still nearer the oscillator, no sound is audible from the oscillator, even when listening in a silent background, and no sound due to direct electrical pick-up from the apparatus can be detected in the telephone. Thus, owing to absence of noise from the apparatus, all intensities down to the lowest can be measured by one observer.

To sum up, the procedure which the authors have adopted is as follows. The observer stands in a representative position in relation to the source of noise, and with the telephone on one ear presents the other towards the source. He then sets the oscillator voltage to the calibration value and adjusts the telephone note by means of the coarse attenuator until it is obviously louder than the noise. He then readjusts until it is definitely quieter, and by successively smaller adjustments decreases the interval between the two settings until an equal small change in either direction causes the two sounds to predominate alternately. The balance reading in equivalent 800-cycle decibels is read directly from the attenuator scales. The procedure is now repeated with the ears interchanged. The mean of the two readings can then be converted into loudness by means of Fig. 7.

Practised observers usually agree to within ± 1 decibel at 100 decibels, the spread gradually increasing to ± 3 decibels at 50 decibels. In important measurements, a more representative value may be obtained by taking the mean of the results of several observers.

It has been suggested that aural balance methods require critical and experienced observers, and are conditioned by differences in individual thresholds. Table 10 shows the results of first attempts by 7 persons, some of whom have abnormal thresholds.

TABLE 10.

First Attempts at Noise Assessments. Equivalent 800-*cycle decibels. Subjects with sensibly equal ears.*

Subject	Threshold displacement	Assessment	True value (practised observers)	Error
A	7	80	80	0
A	7	75	77	− 2
A	7	45	45	0
B	20	76	79	− 3
C	2	82	80	+ 2
C	2	75	77	− 2
D	2	82	80	+ 2
E	− 2	87	85	+ 2
F	2	100	102	− 2
G	6	75	77	− 2

It is seen that, in spite of the misgivings often expressed, the values obtained by beginners, even by those who are slightly deaf, do not deviate seriously from those obtained by practised observers. It was also thought to be of interest to ascertain what results would be obtained by persons deaf in one ear. Table 11 shows results obtained by 2 such subjects on the noise of a high-speed ventilating fan. R and L signify right and left ears covered by the telephone. It is seen that even in these extreme cases the results obtained are not grossly in error.

The aural balance method has occasionally been criticized adversely, in the authors' experience always by persons who either have not attempted to use it or have not given it serious consideration. The method certainly presupposes a desire to obtain an unbiased result, and Table 10 shows the degree of con-

TABLE 11.

Subjects with Unequal Ears. Equivalent 800-*cycle decibels.*

Subject	Threshold displacement		Assessment			True value (practised observer)	Error
	R	L	R	L	Mean		
H	54	2	97	77	87	93	− 6
H	54	2	55	9	32	38	− 6
J	44	20	91	87	89	93	− 4
J	44	20	50	20	39	38	+ 1

sistency which can then be obtained without previous experience. Nevertheless, practice is desirable, as with any other kind of measurement. It is not to be expected that complete unanimity will ever be attained, since a given stimulus may not produce identical sensations in all persons. Nevertheless, the deviation between different practised observers is so small that with quite a small number of observers a result sufficiently representative for engineering purposes may be obtained. As a result of their consideration of the subject, the authors have come to the conclusion that the only method of noise measurement which is trustworthy for general use is an aural comparison method, which involves direct reference to the final arbiter, the human ear.

(5) The Application of Noise Measurements to Engineering Problems.

In this Section a number of questions are dealt with which are frequently raised by engineers and others concerned with noise problems.

(a) *Typical Loudness Values.*

One point of interest is the position assigned to common noises on the decibel and loudness scales. This is illustrated in Table 12.

Columns (2) and (3) have the most practical interest, but it is instructive to note how they are related to columns (4) and (5). The pressure values given in column (4) are those 800-cycle field pressures which would produce the same loudness sensation as the complex noise on an observer facing the source. Column (5) gives the sound energy flow per cm² in an equally loud 800-cycle field. The values in column (5) are derived from those in column (4) by the relation

$$W = \frac{p^2}{dv} \times 10^{-7}$$

where W = rate of flow of energy in watts per cm², p = r.m.s. pressure in dynes per cm², d = density of air, and v = velocity of sound in air in cm per sec.

The decibels above threshold are given in terms of the pressure by the expression $20 \log_{10} (p/p_0)$, where p_0 is the threshold field pressure for 800 cycles per sec., viz. 0·000215 dyne per cm². The relation between the loudness and the decibel values is given by Fig. 7. The inconvenience of using pressure or energy values as an indication of loudness is illustrated by the immense ranges involved.

Turning to more practical considerations, it will be noticed how cramped the decibel scale appears. The figures do not differ very much for noises which, from common experience, differ greatly in the sensation of loudness they produce. To take what might be considered an extreme example, the decibel values would imply that the two circular saws make only 3·7 times as loud a noise as a watch at 3 ft., whereas on the subjective loudness scale the ratio is 160, a figure which, at least, is more in accord with common experience. Undoubtedly persons experienced in noise measurements make mental allowances for the peculiarities of the decibel scale as a scale of loudness, but the average engineer, client, or other person concerned with noise questions, cannot be expected to do so.

The examples given in Table 12 are by no means extreme, as, in electrical engineering, noise problems arise on apparatus ranging from large turbo-generators to watt-hour meters. Any useful loudness scale must therefore apply over this large range. They illustrate the kind of difficulty encountered by an engineer who attempts to express loudness in a quantitative manner by means of the decibel scale. The difficulties are further increased when he attempts to convince a client of the validity of the results. The authors have no desire to deprecate the use of the decibel scale, but only to suggest that its use should be confined to matters to which it is applicable. It is not a scale of sensation, but only a logarithmic scale of stimulus. There is no question of its utility for other purposes, such as in dealing with the attenuation of sound due to buildings, a branch of architectural acoustics of considerable importance in engineering, and in many other instances.

Table 12.

The Magnitudes of Common Noises on Various Scales

	Loudness	Equivalent 800-cycle magnitudes		
		Decibels above threshold	Field pressure	Energy flow
(1)	(2)	(3)	(4)	(5)
			r.m.s. dynes per cm²	watts per cm²
Two circular saws at 3 ft.	160	110	73	13×10^{-6}
Loud motor-horn at 100 ft.	100	100	23	13×10^{-7}
In suburban steam train, window open	50	84	3·6	3×10^{-9}
Ordinary conversation at 3 ft.	20	69	0·65	1×10^{-9}
In quiet saloon motor-car	10	59	0·19	9×10^{-11}
Quiet electric motor (2 h.p.) at 3 ft.	5	49	0·065	1×10^{-11}
Ticking of watch at 3 ft.	1	30	0·0073	13×10^{-15}
Threshold of hearing	0	0	0·00022	12×10^{-17}

(*b*) *Effect of Number of Sources.*

The total effect of a number of sources operating simultaneously has often to be considered in practice.

It has been suggested that since two similar sources emit twice as much sound energy as one, and since the number of decibels difference is given by $10 \log_{10} (W/W_0)$, where W and W_0 are the energy levels in the two cases, an increase of 3 decibels is caused whatever the initial level: also, that each further doubling of the number of machines would add another 3 decibels. This is probably true for the equivalent 800-cycle tone as well as for the actual tones emitted by sources which can be regarded as superimposed point sources emitting pure tones in the range 800 to 4 000 cycles per sec. This simple rule cannot apply to the equivalent 800-cycle tone outside that frequency range, as is evident from Fig. 2. The issue is further complicated in the case of a noise containing a harmonic range of frequencies, e.g. transformer noise. Therefore, in estimating the joint effect of a number of sources, the problem is so complex that experience must be relied on rather than computation.

(*c*) *Effect of Distance.*

In considering the effect of distance from the source on the sensation of loudness experienced, it should be borne in mind that the effect is simple only in the case of a point source emitting a pure tone of a frequency between 800 and 4 000 cycles per sec. in free space with zero background noise. Under point-source free-space conditions the acoustical pressure varies inversely as the distance from the source. The effect of doubling the distance is therefore to reduce the level by 6 decibels, whatever the initial level. Only over the range 800 to 4 000 cycles per sec. will the equivalent 800-cycle tone change by this amount. Table 13 shows the manner in which loudness falls off with distance for an 800-cycle tone of initial intensity 72 decibels at a distance of 1 metre.

TABLE 13.

Distance	Decibels	Loudness	Percentage of initial loudness
metres			
1	72	25	100
2	66	17	68
4	60	11	44
8	54	7·3	29
32	42	2·8	11
128	30	1·0	4
512	18	0·3	1·2
4 096	0	0	0

It is seen that the rate of decline of level in decibels with distance is relatively small, whereas in reality loudness declines substantially. Also it is deduced that, under the conditions assumed, the sound would be just audible at a distance of $2\frac{1}{2}$ miles. In practice this figure would be greatly reduced by the inevitable background noise, as will be discussed later. Supposing the background level were 40 decibels, a quite moderate figure, the source might be inaudible at 512 metres, hardly distinguishable at 128 metres, and not specially obtrusive at 32 metres. With non-spherical sources, the simple inverse law with respect to pressure holds only for distances large compared with the dimensions of the source. At shorter distances the pressure may fall off more slowly at first, later following the simple law. With complex sounds the conditions will be again modified. Assuming zero background, on proceeding away from the source the various components will fade out in succession as their threshold pressures are reached, the order in which the components disappear depending on their relative threshold pressures and initial intensities. Thus the character of the sound heard will vary with distance, the ultimate sound being a pure tone. Bearing these limitations in mind, however, Table 13 illustrates the kind of effect that may be expected and shows that, if loudness values are to be measured to 5 per cent accuracy, the uncertainty in the distance from the source should not exceed 15 per cent. If the source of sound is placed in an enclosure, the conditions may be profoundly changed. The only condition under which the simple laws would hold would be in an enclosure with highly absorbing walls,* where the reflected sound energy would be negligible. Even with a moderate amount of reflection, the conditions would be far from simple. The conservation of energy by the highly reflecting walls of an ordinary brick substation is quite marked in effect, the level inside the building being in some cases increased by 5 decibels compared with free-space conditions.

(*d*) *Effect of Enclosures.*

It is not necessary to emphasize the fact that, in the case of a source placed in a building, the noise audible to a person outside may be totally different, both in composition and in amplitude, from that heard inside. The attenuation of sound through the walls of a building depends, in general, upon the mass per unit area of the walls. An air space does not in itself cause appreciable attenuation except so far as it constitutes a discontinuity between two walls; hence the thickness of the air space is immaterial. With suitable design, the attenuation afforded by a building may be 40 or more decibels. Thus if an 800-cycle tone of 80 decibels is set up within, the resulting level outside may be only 40 decibels. The corresponding loudness values are 41 and 2·4 respectively, showing that a large reduction in loudness can be obtained by a suitably designed enclosure. A discussion of the design of enclosures for noise-reduction purposes is beyond the scope of the present paper, but in many cases it is along these lines that a practicable solution to noise problems in distribution apparatus is to be found.

(*e*) *Effect of Noise Background.*

A subject which is of vital importance in making noise measurements on machinery and in considering how much noise it is permissible for a machine to make in a given situation, is the background of noise present. The

* *Engineering*, 1933, vol. 135, p. 563.

sound sensation from a given source perceived by a listener may be greatly modified if another source is introduced. It is a matter of common experience that one sound can "drown" another. Suppose a source A which is being investigated is operated in an enclosure where no other sound is present. A listener in the enclosure will then receive an impression of loudness in accordance with the output of the source. If now A is turned off and another source B introduced, the listener's threshold may be raised by B owing to the de-sensitizing action of the ear, so that if A is now turned on, as well, it may not produce as great a sensation of loudness as it did in the absence of B. In other words, A is partially masked by B. In order that an appreciable effect may be produced, however, B must be comparable in magnitude with A. With certain frequency relations, if B is made sufficiently large compared with A, A will become indistinguishable or will be completely masked by B. The relative intensities necessary for complete masking depend on the composition of the sounds in question, and to some extent on their order of level, but experience shows that the necessary difference varies from 15 to 30 decibels for common noises. Taking a figure of 20 decibels and supposing a noise under observation is of 80 decibels, some error might arise in the measurement when the background noise exceeded 60 decibels. When the background exceeds 70 decibels the conditions are unsuitable for measurement. When making noise measurements it is therefore essential to ascertain whether any extraneous noise present is of sufficient intensity to cause error.

Extraneous or background noise plays an important part in the specifying of the noise it is permissible for a machine to make. Clearly the obtrusiveness of the noise made by a given machine can be greatly modified by the masking effect of background noise. Table 14 gives a number of examples of background noise in various surroundings. Obviously it is useless to specify specially quiet operation in a motor intended for use under conditions approximating to 85 loudness, or 95 decibels, of background. Almost any motor would be unobjectionable, if not inaudible, in such surroundings. Except at close quarters, a motor would not be clearly audible under these conditions unless it gave an intensity comparable with, or greater than, this amount.

The case of apparatus installed near a main street of a busy city is another common example. If it can be assumed that the district is non-residential and that

TABLE 14.

Typical Noise Levels in Various Surroundings.

		Decibels above threshold of equivalent 800-cycle note	Loudness figures
Factory	Erection shop	90–95	67–82
	Steel-tank manufacturing shop	100	100
	Wooden-box shop	95	82
Power station	Machine room, near large turbo-sets	100	100
	Boiler house (mainly steam hissing)	90	67
	Fan room (mainly noise from boiler house)	90	67
	Pump house	102	109
Streets	Busy main street of city	about 70	22
	Ditto, with trams passing	85	53
	Office on street, weekdays		
	Ground floor, windows open	70	22
	Ground floor, windows closed	60	11
	5th floor, windows open	60	11
	5th floor, windows closed	50	5·5
	Office on street, Sunday morning		
	Ground floor, windows closed	25	0·6
	5th floor, windows closed	15	0·2
	Horse cart on cobble stones at 50 ft.	60	11
	Automobile accelerating at 150 ft.	65	15·5
	Street in residential district, day	50	5·5
	Street in residential district, night	30	1
Miscellaneous	Train, dining car, 60 m.p.h.	80–90	41–67
	Ditto, in tunnel	95	82
	Locomotive blowing off steam (100 yards)	70	22

periods out of office hours do not matter, we have to consider a background varying between 22 and 53 loudness or 70 and 85 decibels. If the noise emission were limited to 70 or even 75 decibels at 1 or 2 metres from the source, the noise would be unnoticed for most of the time at greater distances. On the other hand, if a neighbouring hotel has to be reckoned with, greater precautions will be needed, combined with a consideration of how far distance and the screening effect of adjacent buildings are likely to be of assistance. At certain hours of the night the background may be as low as 1 loudness or 30 decibels, so that an altogether lower level of noise emission would have to be aimed at. The same conditions arise in quiet residential districts, and, unless apparatus of entirely abnormal design is to be used, some form of enclosure is required if the problem is to be adequately dealt with.

In conclusion, the authors wish to record their appreciation of the ready co-operation of members of the staff of the Metropolitan-Vickers Electrical Co. during the course of the research, particularly Mr. A. S. Ennis, who carried out much of the experimental work, and those who patiently spent considerable time and trouble on the observations on the properties of the hearing system. Their thanks are also due to Mr. A. P. M. Fleming, C.B.E., M.Sc. (Director of Research), Mr. J. S. Peck (Director and Chief Electrical Engineer), and Mr. K. Baumann (Director and Chief Mechanical Engineer), for their continued interest and support and for permission to publish the paper.

Discussion before The Institution, 8th March, 1934.

Dr. A. H. Davis: The authors have dealt comprehensively with a particular aspect of the noise problem. They have redetermined the threshold of hearing, and their results agree largely with recent work by Sivian and White; they have checked Kingsbury's curves relating to pure notes of equal loudness; and they have arrived at conclusions, in regard to the instrumental technique of aural measurements of noise, with which I am in general agreement. Towards the end of the paper they deal with the evolution and value of a purely subjective scale of loudness. It would be useful to know how the particular scale they have evolved compares with similar scales which have been set up by other observers in the same field; and it will be interesting to see whether an agreed scale would be helpful. At the National Physical Laboratory we have varied contacts with the noise problem. We have worked on noise in aircraft, trains, omnibuses, streets, offices, factories, flats, and hotels; the noise of vacuum cleaners and motor horns; and we have measured the transmission of noise through partitions, floors, ducts, silencers, and exhaust systems. In almost all cases the object of noise measurement has been noise suppression, and we have found an audiometer scale on a logarithmic basis (the decibel scale) to be convenient. In Section (5) the authors say that they "have no desire to deprecate the use of the decibel scale, but only to suggest that its use should be confined to matters to which it is applicable." With this one would agree, but it is equally true that the use of their subjective scale should also be confined to matters to which it is applicable. It is possible to challenge the authors' statement that the decibel scale "is not a scale of sensation, but only a logarithmic scale of stimulus." Surely, if the purely subjective scale is a scale of sensation, so is the decibel scale; for Fig. 7, in showing a relation between the two scales, implies that if one is a loudness scale the other must be a loudness scale also. I am hesitant to agree with the authors' apparent claim that their subjective scale gives the layman a better idea of loudness problems than the decibel scale. For one thing, it will not be trouble-free. People will ask whether, on the subjective scale, the sound emitted from two identical sources will be double the sound from one. The answer is, "No." Nor is there any simple relation. On the decibel scale doubling the distance means subtracting a certain number of decibels from the loudness, according to the nature of the sound, but there is no such simple relation on the subjective scale. Again, the authors describe two subjective scales, one built up of perceptible increments in loudness and another based on assessing mentally when one sound should be regarded as twice as loud as another. The authors have rejected the incremental scale, and have selected, as more helpful, the one based on mental ratios. They will, however, probably encounter persons who find the rejected conceptions more helpful. For instance, last night I noticed that a ticking clock—unit loudness on the authors' scale—was about as loud as a gas fire, and that, by turning the control tap, I could reduce the loudness in 10 quite distinguishable steps. Thus 1 unit of loudness was divisible into 10 increment steps, and questions may well be raised about the magnitude of those steps on the authors' scale. No scale is free from misinterpretation, but some are more useful than others. I suggest that the following are the points in favour of the decibel scale. Nature works more or less on a logarithmic basis in the transmission, attenuation, dissipation, insulation, and absorption of sound. On the decibel scale a partition between two rooms will reduce the loudness of a sound by a certain number of decibels, largely (if not quite) independent of the initial decibel value. With the authors' scale, on the other hand, the position is more complicated. A partition might reduce a certain loud noise by 30 loudness units; but if the sound had been initially one-fifth as loud the partition would only have reduced the loudness by 9 units. To take other examples, ventilation ducts and speaking-tubes give an attenuation of a certain number of decibels per foot run; tuning-fork decay is nearly constant when measured in decibels per sec.; and insulating supports and exhaust silencers tend to reduce the loudness of noises by a fixed number of decibels. No such tendency to constancy would appear if the authors' subjective scale were employed. Engineers will almost certainly use the decibel scale whenever they propose to act on the measurements obtained, and the introduction of a purely subjective scale as well may lead to confusion. Measurements made at the N.P.L.—either by means of an audiometer calibrated in decibel steps, or by means of a tuning fork, using a method which I suggested some time ago—show

that sound becomes painful at about 120 decibels above threshold; that a pneumatic drill gives a noise level of about 90 decibels above threshold; trams, omnibuses, and trains, give levels of the order of 60 to 70, a quiet restaurant 40, a clock ticking 30, a quiet garden 20, and a whisper about 10. These numbers enable one to obtain a good idea of what is meant by any given loudness on the decibel scale, and make interpretation by laymen reasonably simple. For instance, the Aeronautical Research Committee, in reducing the noise of aircraft, found that the noise level of some of the machines with which they had to deal was about 110 decibels—an almost painful level. In others it was 90 decibels; this was quieter, but still in the region of the pneumatic drill. They held before them the aim of reducing the noise level to that in a train (60 or 70 decibels), and had, in the decibel scale, numbers which revealed the progress made. From this point of view I see no great difficulty about the decibel scale, provided that a certain number of everyday measurements are available to enable a layman to interpret it. I should be glad to have the authors' views on my suggestion that the usefulness of logarithmic scales is so great that they are certain to be used, and that it would perhaps be confusing, rather than helpful, to give prominence also to a scale based upon purely subjective estimates. Finally, although I strongly support the free use of a logarithmic scale of equivalent loudness, such as that called the decibel scale, I am equally strongly of the view that the terminology at present employed is unfortunate and misleading. There are two different quantities, each called a decibel. One relates to power ratios in the noise itself, the other to power ratios in a note heard in the telephone earpiece of an audiometer, and judged to be as loud as the noise. The ratios are not necessarily the same, and they should not both be expressed in decibels. I suggest that when purely logarithmic power ratios are concerned, i.e. in connection with intensity, a decibel change could with advantage be called a decibrig—after Briggs, who invented logarithms. On the other hand, a loudness-change corresponding to a 1-decibrig (or 1-decibel) change in the intensity of the note in the telephone earpiece of the audiometer could (following German practice) conveniently be called a "phone" (or "phon"). Thus a phone (or phon) would be the loudness change associated with a 1-decibrig change in the intensity of the audiometer note heard in the telephone earpiece. A nomenclature such as this would eliminate the present double meaning of the decibel, which is disastrous to an easy understanding of the important subject of noise measurement and control.

Mr. W. West: The collection and presentation of subjective data such as that given in the paper, requires a great deal of skill and patience; a large number of tests on a large number of subjects are usually required, and it is opinions or perceptions rather than facts that are recorded. Even in the determination of thresholds or of minimum perceptible changes, the quantity being measured is under the influence of anything that can affect the subject's powers of perception, and the technique of making the test is itself such an influence. As a matter of principle I regard subjective data as being particular to the technique employed in the tests. As a result of the tests recorded on page 416 the authors conclude that for these tests the technique is unimportant. This may well be the case, but it does not alter the principle that it is unsafe to make such an assumption without sufficient proof of its validity. For these tests it seems that the authors changed only the method of listening, not the method of obtaining a balance, and they employed practised observers and sustained sounds. Much technical literature has been published relating to methods of measuring noise, but little has been said as to exactly why noise is measured in particular cases, and to what use the information obtained from the measurements can be put. We are concerned both with the energy emitted by the source and with effects on hearing, which may be of annoyance or of impairment of hearing. Noise is infinitely variable in time, space, and character, and a wide scope for individual treatment of different noise problems is necessary. The Post Office is concerned with the measurement of noise, primarily because of the masking effect of noise on telephone conversations. This effect can be greatly enhanced when there is severe side-tone, i.e. local transmission from microphone to receiver. Consequently it is now as a matter of routine that tests, such as articulation tests, for gauging the quality of telephone transmission are carried out in the presence of noise. These laboratory tests are made with controlled room-noise, variable in intensity or character at will, and reproduced by loud-speakers.

Our present method of measuring loudness of noise is similar to that arrived at by the authors, except that the standard frequency is 1 000 cycles per sec., and the attenuator is graduated in 5-decibel steps. It is easier to use an audiometer with wide steps, and I prefer to rely on a large number of easily-made observations than on fewer observations working to a narrower margin. We have recently carried out a number of measurements on a controlled noise at different intensity levels by different methods of balancing; wide variations have been found with some methods, even on the average results of several observers. The most consistent results were obtained using a technique similar to that described by Fletcher and Munson,* involving alternate listening to the audiometer tone and the noise. This method measured the noise at a level 2 or 3 decibels lower than the best results obtained with simultaneous listening. I note that in Table 8 a comparison between the simultaneous and alternate methods of listening shows the same tendency. I contemplate basic loudness measurements of controlled sounds in the laboratory by a suitable subjective method, using several observers, and the use of the controlled sounds for calibrating apparatus —whether subjective or objective—suitable for general use for loudness measurements. If the controlled sound is sufficiently similar in character to the noise to be measured, there is much to be said for an objective measurement, and claims have recently been made for the performance of certain objective apparatus in imitating aural loudness balances for a great variety of types of noise. In particular I would mention the recent work of Steudel,† who has studied loudness on the basis of clicks,

* *Journal of the Acoustical Society of America*, 1933, vol. 5, p. 82 (Appendix A).
† *Hochfrequenz-technik and Elektroakustik*, 1933, vol. 41, p. 116.

which, when repeated with sufficient rapidity, become a sustained sound. He concludes that the wave-form is important in regard to loudness, i.e. for complex tones the phases matter as well as the magnitudes of the component simple tones. Does not this account for the failure of the "summed equivalent energy" computations shown in Table 3? Certainly our studies of clicks in telephone receivers have shown that, for different types of clicks, the loudness and energy are not related.

Mr. A. B. Howe: I am particularly interested in the authors' curves relating loudness to stimulus expressed on the decibel scale, because, for example, in trying to estimate what increase of power to a loud-speaker would be required to produce a given desired increase in loudness I have found that the bases for estimation provided either by the Weber–Fechner law or by the possible direct relation between loudness and energy are unreliable. It remains to be seen whether the curves shown by the authors in Fig. 7 form a more reliable basis. As regards the reproduction of music, this is somewhat doubtful because the authors themselves show that caution is necessary before conclusions can be reached, on account of the disturbing effect of combinations of sounds of different frequencies, particularly when these are in harmonic relationship. With regard to the properties of the oscillator which is employed to provide the standard tone, I note that filament control is used to get a fine variation of output. I should like to ask whether changing the filament temperature in this way does not vary the harmonic content of the tone. A condenser microphone is used for investigating the loudness of sounds, and the measurements are made in reverberant surroundings where sounds reach the microphone from all directions. Are the polar diagram of the microphone, and its relation to frequency, taken into account? As part of the sound energy reaching the microphone is reverberative and part comes directly from the source of noise, it seems to me that the direction in which the sensitive surface of the microphone is pointing must have a measurable effect on the results obtained. I am interested in the statement that deaf or partially deaf persons using the audiometer obtained practically the same results as those with normal hearing. I can quite understand this if the people who were making the measurements were equally deaf over the frequency range, but tone deafness is, I think, a well-authenticated phenomenon. For example, a person who happened to be particularly deaf over a range of frequencies including the standard reference tone of 800 cycles per sec. would surely obtain erroneous measurements if he were observing sounds the frequencies of which were outside his range of deafness. The statement is made that the sound level inside an ordinary brick substation is in some cases increased by 5 decibels compared with free-space conditions. The Research Department of the B.B.C. have made some rough calculations in relation to the ratio between the direct sound reaching the microphone in a studio, and the reverberative sound coming from the walls, floor, and ceiling. The studio had a volume of about 125 000 cub. ft., and at a distance of 20 ft. from the source the calculated value of the indirect sound intensity was approximately 5 times that of the direct sound, or 7 decibels above it. In a studio such as that under consideration the mean sound absorption of the walls is of the order of 20 per cent, whereas in a brick substation 5 per cent would be more nearly the correct value. In the latter case the reverberative sound intensity would be 4 times as much, so that the figure would be 13 decibels, and the sound intensity would therefore be 20 times that under free-space conditions. I am surprised that the authors should suggest a figure so low as 5 decibels. Our experience confirms the statement that the mass per unit area of the wall is the main factor in providing freedom from sound transmission. To obtain soundproof conditions we use walls of very solid brickwork. Where this is impossible, an improvement in the soundproof qualities of a light partition can, however, be achieved by the use of some of the absorbent materials, such as mineral wool, which are commercially available.

Mr. W. E. M. Ayres: The authors appear to have viewed the question of noise measurement entirely from a subjective point of view. They pay considerable attention to measurement of the threshold of hearing, which is of little practical importance. I should prefer to eliminate the subjective term and speak of an "agreed threshold," which might be any properly defined physical unit, reproducible in any well-equipped laboratory. Similarly on page 402 the authors state that "the former seemed much more than 'twice as loud' as the latter" (referring to 90 and 45 decibels respectively). Why should this not be the case? The decibel scale is a physical logarithmic scale of sound pressures, not a subjective scale of loudness values. It would be as reasonable to complain of a thermometer scale because 90° C. feels more than "twice as hot" as 45° C. When so many acoustic phenomena are quantitatively logarithmic, there is considerable reason for retaining the decibel unit, and incidentally the authors have made the whole of their paper intelligible in terms of such a unit. The proposed loudness scale has the unfortunate result of transferring the alleged compression into the most useful part of the scale. Fig. A shows side by side the "noise thermometer" reproduced from Dr. Kaye's paper* and (on the right) the same transferred to the proposed loudness scale. Some dotted lines have been drawn across the diagram in the 40–50-decibel region, representing a few complaints concerning electrical apparatus in residential districts. It will be seen that these would become unintelligible in a very congested region on the proposed loudness scale. The only use for this loudness scale is as a side-by-side comparison to explain average subjective values to persons otherwise unfamiliar with acoustical matters. When the authors come to noise measurement they recommend universally an aural balance method at 800 cycles per sec., yet they admit that this "presupposes a desire to obtain an unbiased result." One cannot, however, make such assumptions in cases of dispute. Mr. West stated that the controlled noise should be of the same type as the noise to be measured, and this agrees with my experience. For complex noises I prefer to test against a complex tone; but in this case for practical (not laboratory) purposes I find masking to give the most consistent results. On the other hand, when balancing a complex tone against

* Report of the British Association, 1932, p. 356.

a pure tone noise, or a pure test note against a complex noise, I find that an aural balance is better. When this type of test is employed identical readings can be obtained by trained and untrained observers. Tests of this sort have settled disputes of 6 months' standing at one demonstration. The psychological aspect, however, has not been settled. Whatever one may do to persuade a person about the audibility of a disputed noise, it is impossible to persuade him that it is less annoying—and here the questions of general health and willingness to agree become prominent.

Mr. D. B. Hoseason: My work has to do with heavy rotating machines, and for the last 5 years I have been doing my best to use the decibel scale. As the result of my experience I wish to support strongly the use units vertically, I find that 80 decibels correspond to about 62 arbitrary units and 60 decibels to 39 arbitrary units. Comparing this with Fig. 7, I find 40 of the authors' loudness units instead of 62, and 10 instead of 39. It is disconcerting to find that the two scales of sensation do not give the same result, and I should like to ask the authors what are the grave psychological objections to Fig. 4 which they mention. An important point in favour of Fig. 7 is that the numerical values of the standard and mean deviations from 25 loudness units and upwards (Table 2A), are of the same order as those for the threshold of audibility given in Table 1. As most of the previous speakers seem to accept the threshold of audibility, and as the standard of deviation is much the same farther up the scale, what

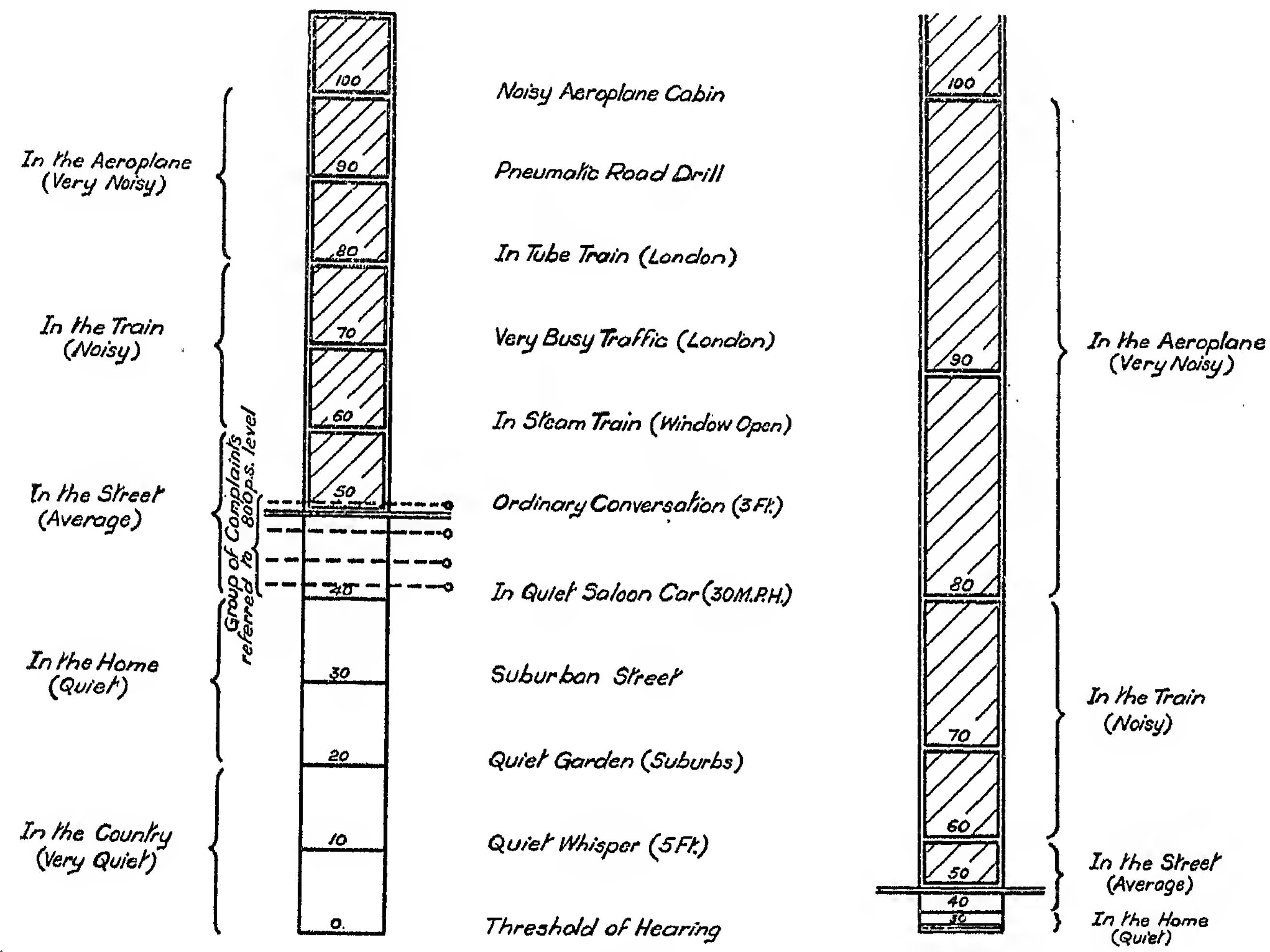


Fig. A.

by engineers, when dealing with laymen, of a loudness scale based on sensation. The opposition of previous speakers to Fig. 7 is, I think, based on the fact that it is a scale of sensation and is therefore not absolute. I suggest that the decibel scale is also in reality a scale of sensation, because its zero point, the threshold of audibility, is a sensational point. If the threshold value is accepted from tests of sensation, why cannot we accept the rest of the sensation curve? I know that in actual fact it does not matter where the threshold is set, but we use the words "threshold of audibility" to give the layman some idea of where the scale starts, and I suggest that it is wrong to build a scale of stimulus on a foundation of sensation. With regard to the numerical values of Fig. 7, I have tried to see why Fig. 7 should not compare with Fig. 4. By replotting Fig. 4 with 100 decibels on the base line corresponding to 100 arbitrary objections can there be to using a sensation scale over the part in which we are interested? High-speed motors and generators are particular offenders in regard to noise, and recent improvements in these machines go far to support the use of the authors' sensation scale. To take the specific case of boiler feed-pump motors of the order of 500 h.p. at 3 000 r.p.m., 5 years ago one would have had great difficulty in obtaining a machine of this type with a noise level below 108 decibels. Two years ago such a machine could have been obtained with a level of 102 decibels above threshold. A fortnight ago the figure of 96 decibels was achieved. On the decibel scale this reduction only amounts to 12 per cent; in terms of Fig. 7, however, the boiler feed-pump motor of 5 years ago had 140 loudness units, that of 2 years ago had 107, the present one has 84, and the reduction in loudness is 40 per cent, a figure much more consistent with the

amount of money which has been spent in reducing the noise emitted by these machines.

Mr. E. T. Norris: The problem of noise measurement has only recently come into prominence in electrical engineering, and until then remarkably little progress had apparently been made in regard to solving it. Fig. 7, which concerns loudness units, the fundamental item in the study of noise measurement, is a complete departure from existing ideas on noise measurement, and so far it has not been subjected to violent criticism. With regard to the subjective method of noise measurement that takes up the latter part of the paper, it seems to me that in Table 10 the authors are either unduly optimistic or extremely lucky. I find that ordinary people, using one of these instruments for the first time, cannot obtain readings even within 2 or 3 decibels. This subjective method of noise measurement is well adapted to the measurement of what are relatively point sources of noise, such as vacuum cleaners or motors, but it is not nearly so easy to apply it to noise sources of large dimensions, such as transformers. By walking round a transformer and taking readings very close to it with an instrument of the type used by the authors, which is often all that can be done when dealing with a transformer in service, one does not measure the noise emission from the transformer in the sense in which local residents are interested in it. Such readings merely reveal the local characteristics of the tank in regard to noise emission. I suggest that the distance of the observer should be at least 6 times the greatest linear dimension of the elevation of the object under investigation. Even at this distance, one is still only measuring the noise that is radiated from the transformer horizontally at the average height of a man's ears, and this is not usually, in the case of apparatus installed in the open air, what is of importance; very often other apparatus, or low walls, cut off the noise in that direction. Some years ago we made a study of this problem and came to the conclusion that the best and most suitable standard position for measuring noise was 25 yards away and 15 feet high. Noise is usually the most objectionable to local residents, and particularly so at night when the residents are in bed. The 15 feet represents the average height of a bedroom. The distance is not important, but the height is; because when one is measuring noise near the ground very close to a large object such as a transformer, one does not receive any of the noise emitted from the cover, an all-important factor in the case of a bedroom or other position high up. The apparatus we finally adopted for measuring the noise emitted by large objects embodies, as the noise-sensitive device, a microphone mounted on a tripod. The apparatus forms a complete noise-analysing and measuring set; it is portable, and as far as possible is always installed 20 to 25 yards from the transformer. It automatically provides the standard height.

Mr. R. A. Bull: I am particularly interested in the authors' statement to the effect that they are not aware of any valid method for the evaluation of subjective loudness, assuming that the constituent components of the noise are known. It is probable that, for the purpose of discussion, the ear can be considered to act in the following manner. When presented with a complex noise the aural mechanism behaves as an analyser, reduces the noise to its simple components, assigns to each component a magnitude of auditory sensation, and finally sums these magnitudes arithmetically. The final summation is what we call loudness. The magnitude of the assigned auditory sensation for any component is dependent on the absolute frequency, the absolute intensity level, and the position of the component in relation to other components. It therefore appears that if we could obtain relationships that would permit us to evaluate the magnitude of the auditory sensations, we should have available a method for the calculation of subjective loudness. Such a method of evaluation was given in a recent paper by Fletcher and Munson,* and some experimental work in this country has shown their process to be valid. Thus by analysing a steady complex noise, applying the magnitude of the auditory sensation for each individual component (taking into account masking), and finally summing these magnitudes arithmetically, we are able accurately to evaluate loudness. I should like to ask the authors whether they have considered using the scale of Fig. 7 as a method of evaluating loudness along these lines, and whether they obtained agreement with subjectively observed loudness. If it is true that when considering a complex noise the ear sums the magnitude of the auditory sensation of each component, then it would appear that evaluations of subjective loudness by arithmetical summation of the intensities of the components would lead to erroneous results. This may be a possible explanation of why the authors obtain large discrepancies between summed equivalent-energy calculations and subjectively-observed loudnesses, for tones having the components in harmonic relationship. For example, it is noticeable in Table 5 that the greatest differences between the summed equivalent-energy column and the subjective-observation column occur with a large number of harmonic components present. If these components are small on an energy basis compared with the fundamental, their contribution in the calculation is negligible, but their contribution on the basis of the magnitude of the auditory sensation may be appreciable. The fact that closer agreement has been obtained with non-harmonic relationship between the components is, I would suggest, not a function of the fact that the components are in non-harmonic relationship, but a function of the small number of components which were present. I should welcome the authors' comments on this point.

Mr. D. A. Oliver: Fig. 7 represents a part of the data required in correlating subjective impressions with objective measurements. At the Research Laboratories of the General Electric Co. we have found it necessary to consider the subjective effect of various objective changes in intensity, whether it be the sound of music in a room or whether it be in the assessment of noise. I should like to mention first the method of building up a scale of numbers purporting to represent subjective loudness, as given in Fig. 4. It seems reasonable to assume that if we assessed each perceptible increment in loudness as a step in the loudness scale we should get a result which ought to agree with the guessed or estimated loudness; but when that process has been carried

* *Journal of the Acoustical Society of America*, 1933, vol. 5, p. 82.

out it is found that the curve of Fig. 4 does not agree with that of Fig. 7. The test of the validity of Fig. 4 is the result shown in Fig. 7. Carrying out the same process on some results given in 1928 by Riesz,* i.e. converting his minimum perceptible-energy steps into decibels and performing the integration, gave a curve substantially the same as that of Fig. 4, which was derived from independent measurements made by the authors. Another remarkable feature was that going up the ladder of perceptible steps at 1 000 cycles per sec. to the 100th arbitrary point, and carrying out the same process again at 4 000 cycles per sec., gave two curves that very nearly coincided. This seemed to be a justification for the use of Fig. 4 for that purpose, but, as we know from Fig. 7, Fig. 4 is not confirmed by experience. The arbitrary numbers given in Fig. 4 are related to the decibels above threshold or sensation level (the "stimulus level," as the authors call it) by a simple square law multiplied by a constant. Turning to Fig. 7, the sensation-level figure, raised to the fourth power, gives a reasonably good fit with the authors' observed curve. This means that the readings of any acoustic meter which is calibrated in the orthodox way on the basis of decibels above threshold could be simply related to the scale proposed by the authors by raising the observed value to the fourth power and multiplying by 10^{-6}. It is interesting to note that the estimate of the rate of change of subjective loudness with the initial intensity of the sound varies as the cube of the sensation level, and therefore the effect of a 1-decibel change at a level of 20 decibels above threshold is only 1/125 of that of a similar change at 100 decibels. Another simple deduction is that the authors' observed curve (Fig. 7) is practically a square function of the step-by-step or integrated-loudness curve.

Mr. A. C. Hutchinson: The authors' contribution to the subject is the most outstanding since the time of Lord Rayleigh, while their scale of numerical values is epoch-making in that it is a psychological measurement as much as a physical measurement. Another outstanding point of the paper is that the summation of the energies in a complex sound does not give a measure of the noise made by that sound. Speaking from the point of view of the mechanical engineer, who is as much concerned with noise as the electrical engineer, I consider that the whole subject has been made rather unnecessarily complex, the decibel scale, as is shown by the example quoted on page 419, being particularly misleading. A further objection to the use of the decibel in sound measurement is that two distinct physical entities are considered, namely sound pressure and energy, and that the decibel is used indiscriminately as a unit for both. As another instance of an avoidable complexity I should like to criticize Fig. 2, which gives the decibel value of each note above its own threshold. Before the results can be easily interpreted it is necessary to add on to each curve its own threshold value; for instance, the 100-cycle curve has a threshold shift of about 24 decibels. Thus, to obtain the true intensity values, one has to add a different number of decibels to each curve. When this has been done by displacing each curve horizontally to the extent of its threshold shift, one obtains the curve shown in the lantern slide, and this demonstrates quite clearly that though the sensitivity of the ear to low notes is low at low intensities, at higher intensities it becomes almost constant, and, in fact, even exceeds that associated with the 800-cycle standard. This point is very significant, and is not well brought out by Fig. 2 in its original form. It has generally been assumed that the ear is very much more sensitive at 800 cycles per sec. than it is at 100, but apparently this is only true below intensities of 50 decibels, which noise level is too low to be of much engineering importance. The firm by whom I am employed recently produced a geared turbo-generator which was almost as noisy as that dealt with in Table 3. We attacked the trouble in two ways; by improving the gear teeth, and by trying experiments with blanketing. The outstanding result of the latter experiments was that enclosing the gearbox in an additional casing of cast iron lined with felt made no appreciable difference, yet, when one then put one's ear against the gearbox casing, no noise appeared to be coming out of it. By means of a stethoscope it was discovered that the noise was not emitted only by the gearbox but came from all over the machine, including the baseplates. This result suggests that noise in heavy machinery starts from blows which take place inside; these set up sound waves in its material which travel throughout the structure until they find convenient radiating surfaces from which to emerge. From tentative theoretical considerations, now confirmed by Fig. 7, it was concluded that even a very large reduction in noise energy, such as was effected by blanketing the whole of the gearbox in this instance, only reduces the loudness level extremely slightly. According to a recent article by Mr. Colebrook,* the loudness figure varies roughly as the square root of the sound pressure, which is equivalent to the fourth root of the sound energy. Whence, if the gearbox emitted 95 per cent of the total sound, its complete blanketing would only just halve the noise, whilst the blanketing of the whole machine would have to be 99·6 per cent perfect to reduce it to one-quarter of its original value. Taking background noise into account provides an explanation of another observation, namely that even if considerable changes in noise appear to take place near the machine, at a distance they are barely distinguishable. It would appear, therefore, that two cardinal principles to be observed in experiments on noise reduction are: (*a*) The results of an experiment must be judged at close quarters; (*b*) If blanketing is attempted it must be carried out very thoroughly, *all* the noise-emitting surfaces being located and covered.

(*Communicated*) The authors' approximate formulæ may be supplemented by the expression

$$\text{Loudness} = 3\,000\,W^{\frac{1}{4}} - 0{\cdot}303$$

where W is the rate of energy flow in watts per cm^2.

which, though very inaccurate for some values, yet holds good over the entire range considered. By plotting log (loudness units) against log (decibels) (i.e. log log energy), as shown in Fig. B, the expression †

* *Physical Review*, 1928, vol. 31, p. 867.

* *World Radio*, 1934, vol. 18, p. 168.

† I am indebted to the authors for the final values of the constants included in this expression.

$$\text{Loudness} = 0\cdot667 \times 10^{-4}[(\text{Decibels}/10) + 2\cdot5]^{5\cdot63}$$

$$\text{or Loudness} = 0\cdot667 \times 10^{-4}(\log_{10} W + 18\cdot421)^{5\cdot63}$$

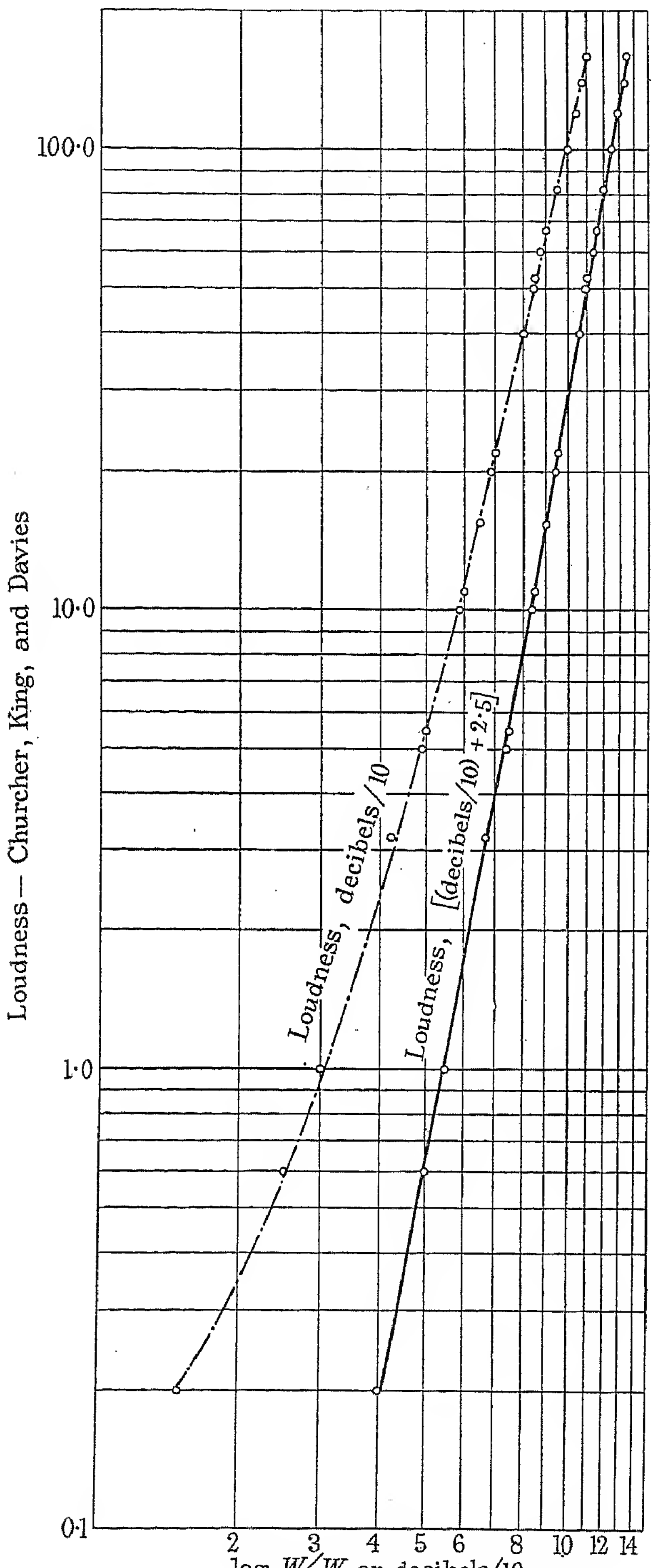


FIG. B.

which fits the results very accurately, may be obtained. This expression implies a finite value (0·012 unit) for the loudness at the threshold of audibility, when $W = 1\cdot2 \times 10^{-16}$, and this conclusion is supported by plotting log (loudness) against decibels (or log W). The fact that the audibility of a sound ceases before its loudness value reaches zero suggests that the authors' scale, though psychologically derived to express *ideas* of noise numerically, may depend upon some physical entity, having its own threshold value, to which nervous sensation is itself proportional. With reference to the over-complication of an already difficult subject by unsuitable nomenclature, I have redrawn in Fig. C the authors' Fig. 2, embodying the unit proposed by Dr. Davis. I submit that by using a unit defined merely as a ratio—i.e. antilog 0·1, or 1·259, which represents a 25·9 per cent

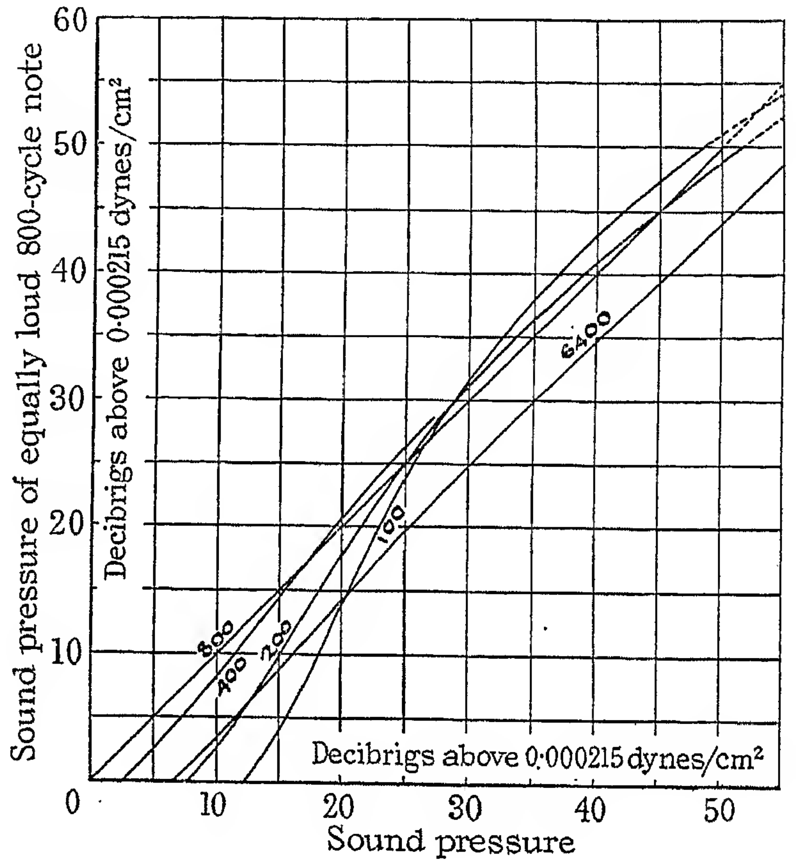


FIG. C.

increase—and by stating the physical quantity which it expresses, all the practical advantages of the decibel scale could be retained and yet its paralysing effect upon first-principle thinking would be avoided.

Lieut.-Commander R. B. Fairthorne (*communicated*): Transformer hum, a favourite example of noise, is of all noises one of the least offensive to the human ear; in fact, it actually has an almost soothing effect on some hearers. I submit that wide fluctuation of intensity or of pitch is the major characteristic contributing to "nuisance." Will the authors say which they consider to constitute the greater nuisance—50 units steadily reproduced, or 30–50 regularly fluctuating?

Dr. L. E. C. Hughes (*communicated*): It is a fallacy to suppose, as several speakers have done, that the 1 000 cycle per sec. decibel scale of noise levels is a linear scale of loudness. This scale has been used for many years as an objective reference scale for noise levels in communication engineering, and there seems to be no warrant for introducing a further subjective scale of loudness levels based on aural estimation by untrained observers, however interesting such a scale may be to

students of acoustics. There is no doubt that an objective industrial measurement of noise levels is needed in this country, such as has obtained for many years in America (where experience did not suggest the necessity of a subjective scale) and to which the authors do not specifically refer. The microphone technique described by the authors is standard practice in any form of acoustic pick-up. The aural-balance method of assessing equality of noises or sounds has often been used since it was first proposed by Cohen, Aldridge, and West.* An alternative to a 1 000-cycle per sec. masking tone, when a masking method is used, is Johnson (thermal agitation) noise. This is uniformly spread over the spectrum and has been used with success in cable interference tests. The authors fail to distinguish between direct noise radiated from a source and reverberant noise in an enclosure, and do not consider the fact that the noise generated by a machine depends materially on the way in which the latter is attached to its foundations. Presumably, what matters is the total sound-power generated by the vibrational surfaces in the instances of machines used indoors. This is readily determined in a calibrated reverberant enclosure when the machine is mounted on pliant springs. If it is erroneously mounted by the customer and so generates noise by conduction, that is not a fault associated with the machine and should be excluded from the measurement. For outdoor installations, the directive sound measurements in a padded chamber are, of course, correct. The problem of noise measurement is not difficult when approached from the communication point of view. As Dr. Kaye has pointed out, the decibel scale of noise-levels is one to be learnt by personal experience, much as we learn the meaning of indication on a thermometer, or a luxmeter. To be a fact and not an impression, noise must be measured objectively.

[The authors' reply to this discussion will be found on page 442.]

Mersey and North Wales (Liverpool) Centre, at Liverpool, 29th January, 1934.

Prof. L. R. Wilberforce: The assessment of a degree of sensation, unlike the measurement of the physical or chemical stimulus which arouses it, is beset with uncertainties. The comparison of the loudness of noises of different character presents a problem similar to that of the comparison of the brightness of lights of different colour, but in the latter the phenomenon of flicker leads to the design of an adequate photometer, while in the former such assistance cannot be invoked. The authors are much to be congratulated, therefore, on having devised a practically applicable scale on which degrees of loudness can be registered and a convenient form of apparatus by means of which the estimation can be carried out. The measure of agreement between the results obtained with this apparatus by different observers, or, more properly, "auditors," is noteworthy.

Mr. R. G. Devey: I think one must accept the technique which has been put forward by the authors, for the simple reason that their experience of sound measurement is greater than that of most other engineers. The value of the paper lies in the fact that it stimulates interest in the reduction of the noise caused by machinery; if manufacturers would take an interest in this subject it would be very helpful and beneficial to industrial works. It is to be hoped that the result of this investigation will be the evolution of quieter machines, and a consequent reduction of the injurious effect of noise on factory operatives. One of the authors' slides showed an observer carrying a portable apparatus and taking a reading of the noise value of a certain transformer; he appeared to be wearing one headphone. Was he estimating the sound intensity by the other ear without a headphone, or was he actually wearing two headphones? If he was only wearing one, the value of the noise must have varied with his distance from the transformer.

Mr. P. M. Hogg: Noise reduction should be the concern of every enlightened works management. In many industrial works the machine operator has to stand still and endure the noise of the machinery for hours on end without relief. He becomes irritated, a certain type of nervous exhaustion sets in, and he begins to take a gloomy view of life and to feel that he is ill-used. Nothing rankles in a man's mind more than a sense of social injustice, and it is well worth while for any works management to investigate the question of the reduction of noise from this point of view. I know of one case where a certain type of gear was adopted rather than a more efficient type, simply because it was the quieter of the two, and in spite of the fact that the lower efficiency meant an additional expenditure of over £1 000 per annum on electrical energy.

Mr. J. O. Knowles: I should like to know whether the threshold pressure varies as the sound intensity is increased or decreased. In Fig. 3 I notice that some reference is made to increasing and decreasing pressure, and the basis of calculation might vary according as the initial value is taken on sound increasing to audibility or decreasing into inaudibility. As an electrical analogy, a neon lamp will not light on increasing voltage until the voltage is considerably greater than the value at which decreasing voltage would put out the glow. With regard to the relation between threshold noise and frequency, it is commonly said that there are silences which can be "felt," and on very low frequencies there are certain noises (like the lowest notes on a powerful organ) which can be felt rather than heard. One of the authors' slides showed a number of contactors in connection with a motor-generator set on which a noise test was being made. A.C. contactors are easily made "industrially" silent, but in connection with buildings, offices, hospitals, and some large domestic work, contactors are used in places where a slight hum is of importance. I should be interested to know whether contactor hum has been investigated by the authors. The problem of contactor noises in domestic or other non-industrial work may be solved in various ways. Careful adjustment of magnet faces or bearings will reduce the hum, or direct current may be obtained through rectifying equipment. A close investigation has shown that vibrations may be present of other frequencies than that of the supply, owing to the natural frequency of such components as contact

* *Journal I.E.E.*, 1926, vol. 64, p. 1023.

pillars. Another practical solution is to use mercury switches or latched-in contactors and thus completely eliminate magnetic hum. The authors state that the artificial ear is not correctly sensitive to harmonics, and I am not sure whether this is regarded as a practical or a theoretical limitation. Could adjustments be made so as to register the peaks on waves only, as in measuring peak voltages, instead of using the r.m.s. values?

Mr. Emrys Williams: The authors have attempted to produce a logical and reasoned objective method of measuring the loudness of noises. In order to do this, they have successfully investigated the relation between the intensity and loudness of pure tones, starting from the hypothesis that what the average individual describes as a sound "of twice the loudness" of a given sound shall be regarded as a sound of twice the number of loudness units. They have extended this relation to embrace sounds of different frequencies, and have redetermined the threshold intensities over a certain range of frequencies, for free-space conditions. Their previous paper* explained how to analyse the intensities of the components of a noise, and they have now applied their intensity-loudness relation to the figures obtained by such analysis, in order to calculate the number of loudness units for the whole noise. It is then a simple matter to calculate the intensity of the pure 800-cycle note which would have the same number of loudness units as the noise. The authors have put this objective method to the test by the simple process of listening to the noise and to the calculated equivalent 800-cycle note, and observing whether they did in fact produce equal sensations of loudness. For certain complex noises the agreement was good, but for certain simple noises there were very serious errors. The intensity of the calculated equivalent 800-cycle note differed by 30 decibels from the intensity of the 800-cycle note judged by ear to be of equal loudness with the noise. Instead of being equal, these two intensities bear a ratio of 1 000 : 1. This discrepancy could not be satisfactorily explained, and the authors therefore decided to discontinue investigation of the objective methods of measuring the loudness of noises. The discrepancy encountered in connection with harmonic scales, however, is probably only the first of a great number of discrepancies. Transient sounds and intermittent sounds would probably result in further difficulties. Nevertheless, since objective methods, when fully developed, are generally of infinitely greater accuracy than subjective methods, it is very much to be hoped that the matter will be pursued further, and in this connection the work described in the earlier sections of the paper is extremely valuable. In examining this work, several questions arise. In the first place, the authors consider two groups of methods of determining the threshold intensity, namely (*a*) the methods of listening for a decreasing sound to become inaudible, and of listening for an increasing sound to become audible; (*b*) the methods of determining the largest sounds whose complete removal, or complete application, cannot be detected. While the authors agree that methods (*a*) give two different results, according to whether the threshold be approached from the higher or the lower intensity, it would appear—from their definition of the threshold as "the largest sound the complete removal or application of which is not detected"—that they consider that the two methods (*b*) give a single result. Above the intensity Q at which a decreasing sound becomes inaudible, the removal of a sound can be detected. Thus Q is the largest sound whose removal is not detected. Below the intensity P at which an increasing sound becomes audible, the application of a sound is not detected. Thus P is the largest sound whose application is not detected. The two groups of methods (*a*) and (*b*) are in essence the same, with the exception that in (*b*) the sound is intermittent, and the ear has an opportunity of comparing silence and sound. Even if it be admitted that, as a result of this opportunity for a reference to silence, the separation of P and Q will be decreased, yet it seems that the psychological causes of that separation (the retention of impressions) still apply. Further, to ensure that under no circumstances the loudness of a sound which has been heard shall be computed to be below threshold, it would seem that the most relevant definition is "the least intense sound which can be detected." The argument that this would involve a finite measurement of sensation is surely invalid, as the determination is bound to be subjective whatever definition is adopted. Secondly, the deviations shown in Table 1 are considerable. If the values for the various observations had been plotted on a threshold-frequency curve—even using logarithmic scales—the points would have been so widely dispersed as to give no reliable indication of the average threshold curve. In view of this the method of averaging becomes of importance, but the authors give very little information on this matter. The reason why the results should be presented for averaging on a scale proportional to sensation rather than stimulus is by no means clear. Moreover, this appears to involve assuming a threshold intensity; for, after explaining that, in the vicinity of the threshold, sensation is proportional to decibels above threshold, the authors state that the results are expressed in decibels from certain defined intensities. Further, since the usefulness of the "mode" of a number of results is dependent on the sharpness or otherwise of the peak, in the curve giving the distribution of the results this curve should be shown. The authors do not differentiate between experimental errors on the part of the subjects and genuine differences of threshold from person to person. How many repeat tests were made with each subject, and was the result of these tests to indicate that experimental errors on the part of the subject could be neglected? Thirdly, on page 417 the authors describe a series of measurements made in order to relate the threshold pressure under free-space conditions to that when a telephone is used. This constitutes a direct check on Wegel's measurements, and it would be interesting to know how the two compare. Finally, in Table 10 it is shown that, using the subjective method of noise assessment developed by the authors, certain persons with displaced thresholds made assessments not differing seriously from those of a normal observer. It might be expected that, for subjects whose whole threshold curve is displaced upwards by a fixed number of decibels, the assessment would be unaffected, but that it would be otherwise for subjects with distorted thresh-

* *Journal I.E.E.*, 1930, vol. 68, p. 97.

old curves. As a result of their tests on 48 subjects, have the authors any information as to whether serious differences occur in the shape of the threshold curve?

Prof. G. E. Scholes: I have no doubt that the authors have chosen wisely in developing the aural comparison method, because the human being is only affected by the impression of noise he receives. The closeness of the agreement between the observations of different observers strengthens the case for the aural comparison method. I have noticed the effect of one sound masking another when carrying out some experiments on the silencing of petrol engines. As the major noises are eliminated, minor and hitherto unheard noises become prominent. I should like to ask the authors whether the portable noise-analysing machine they have developed gives any indication of the source of the noise. It would seem that an analysis of the sound in the sound-analysing instrument would be necessary to indicate the frequencies of the major offending noises and to enable the part of the machine causing the noise to be located. This being so, I should like to ask whether the use of the sound-analysing machine *in situ* is possible. If so, how long would it take to carry out a complete test?

Mr. H. Taplin: In connection with the slide showing the interior of a soundproof motor room we were informed that in order to eliminate sound the motor was surrounded by a 6-in. layer of cotton wool. As the motor in the soundproof room was coupled to a generator in an adjacent compartment, by means of a solid shaft passing through the division wall, it would appear that some material other than cotton wool would be necessary in order to avoid the troubles which invariably follow bad alignment. It would be interesting if the authors would afford some further information as to the motor foundation in the soundproof room.

Dr. J. C. Prescott: The authors have developed an instrument by which comparison may be made between the loudness of any given sound and the loudness of a standard source with a frequency of 800. This comparative method immediately calls to mind the somewhat similar method used in photometric comparisons, and suggests the question as to the meaning of a balance between two sounds of widely differing pitch. The apparatus described in the paper will presumably be applied as an aid to the elimination of noise in machinery, and, as the authors suggest, as a standard in terms of which the maximum permissible noise produced in machinery can be specified. It is here, particularly, I think, that the question arises whether sounds which appear equally loud are equally "permissible." Has any information been obtained as to the relative annoyance caused by notes of different frequencies, and by a composite tone as compared with a pure tone? That noise in machinery is not universally objectionable is shown by a letter from James Watt (quoted by J. H. Marshall) describing an engine which was installed in one of the Cornish tin mines. "The velocity, violence, magnitude and horrible noise of the engine give universal satisfaction to all beholders, believers or not." He was not allowed to remedy this for: "The noise seems to convey a great idea of its power to the ignorant, who seem to be no more taken with modest merit in an engine than in a man."

[The authors' reply to this discussion will be found on page 442.]

North-Western Centre, at Manchester, 6th February, 1934.

Mr. R. Poole: One of the problems which has troubled engineers is the production of noise by ventilating fans, particularly when used for cooling electrical machines. I have been investigating the aerodynamic side of the problem in my own wind tunnel, and have found that great care must be exercised in designing brackets and the like which will fall on the inlet side of a propeller fan. Behind every body placed in an air stream there is a shadow, or region of low velocity, and the strength and dimensions of the shadow depend upon the shape of the body and the velocity of the fluid stream in which it is placed. As the velocity increases, the length of the shadow decreases but its strength increases. Fig. D shows the shadows of three typical obstacles. It will be noticed that for the streamline section at a distance equal to twice its own length the shadow velocity has become almost equal to the velocity of the undisturbed stream. In the case of the cylinder the velocity at 2 diameters away is only 0·5 of the normal, whilst for the square section the corresponding velocity is only 0·3 of the normal. When a propeller fan is rotating behind any such obstacles, the fan blades will be subject to a change in pressure as they pass through the shadow. This variation in pressure is one of the most important sources of noise in ventilating fans. There is another important feature in connection with the flow pattern behind an obstacle; up to the critical velocity the flow pattern is constant, but after this velocity has been passed a double row of vortices is set up which pass downstream. Fig. E shows, first, the constant-flow pattern before reaching the critical velocity, and afterwards, the double row of vortices being carried downstream. If we place in this stream a Pitot tube, such

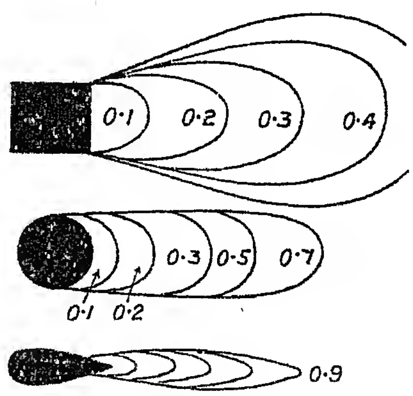


Fig. D.—Typical velocity "shadows" behind obstacles.

as is used for measuring the velocity, and attach a rubber tube, on placing this extension near to the ear the noise set up by the vortices can be heard very plainly. The important feature is that the vortices in themselves produce no serious noise, but on placing the Pitot tube in the stream some of their energy is converted into static

pressure and so a sound wave is produced. We may compare these vortex cylinders with a series of sticks floating vertically in a stream and passing along at regular intervals. If a board or some other obstruction is placed in their path they will impinge upon it with some definite frequency. Similarly when a microphone is used in an air stream it may actually convert noiseless energy into sound waves. The authors have stated that when there is no listener there is no noise; in this particular case the position of the listener or the artificial ear decides whether he will change these vortices or potential noises into an actual sound. There is a distinct difference between the sound set up by the conversion of a vortex stream, and that set up by a normal compression wave. Fig. F shows the expanding throat of a wind channel, and a vortex stream passing down.

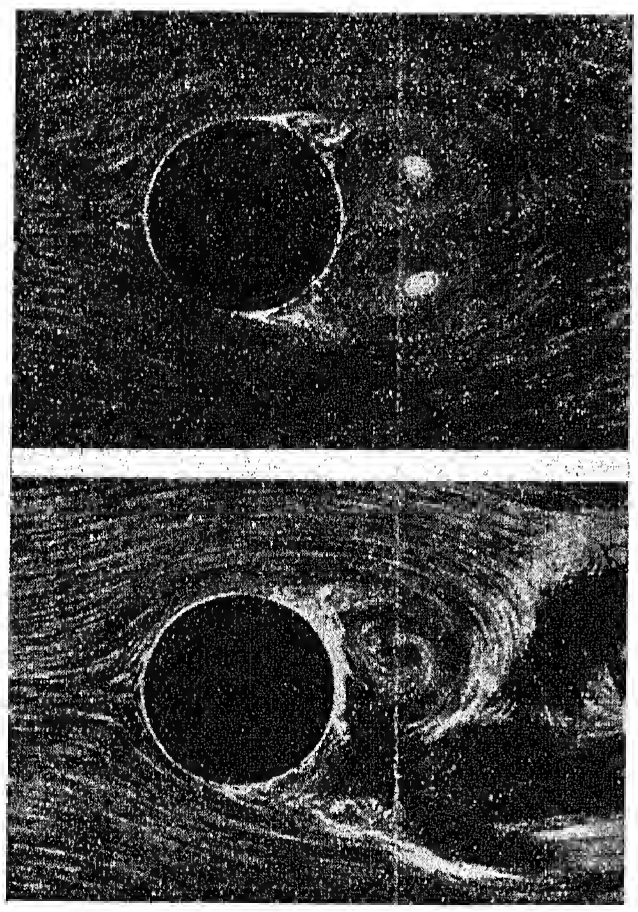


FIG. E.*

Obviously, the frequency with which the vortex cylinders pass a given point depends entirely upon the velocity of the stream at that point. Thus, assuming a normal velocity of 2 000 ft. per min. and a vortex frequency of 500 per sec., the expansion of the throat causes the velocity to fall to 1 000 ft. per min., and the vortex frequency will also fall to one half its original value (i.e. to 250 per sec.). In addition the vortex cylinders act as flywheels having a certain kinetic energy. If they are increased in size, then for the same kinetic energy it follows that their speed of rotation will fall. Eventually, in the open atmosphere, the vortices would die out completely. It is therefore most important to know exactly where the artificial ear or microphone is placed when studying these potential noises. The frequency with which the vortices leave an obstacle depends upon its projected width as well as upon the fluid velocity.

* Reproduced by permission of the Royal Aeronautical Society.

An obstacle 1·5 in. wide, placed in an air stream of 2 000 ft. per min., gives a vortex frequency of the order of 45 to 50 per sec. If the width of the obstacle is halved, the vortex frequency is doubled and becomes 100 per sec., and so on. When the obstacle is on the inlet side of a fan the vortices are superimposed upon the sound made by the fan; and it will be understood

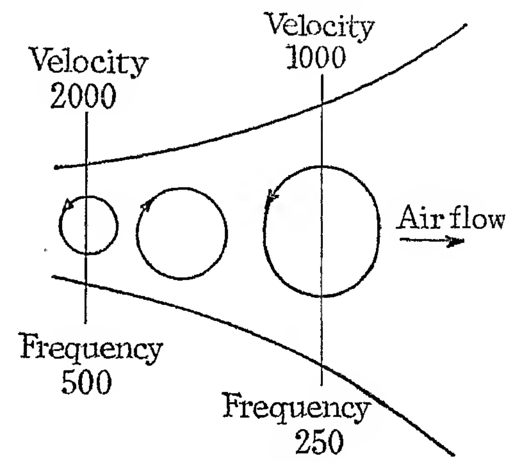


FIG. F.—Change of vortex frequency with velocity.

from reference to Fig. G that the note produced by the vortices striking the fan blades will depend upon the frequency with which the blades pass the object, and the frequency with which the vortices leave the object. Another difficulty enters into the question of noise set up by ventilating fans. If we consider a screw propeller fan, each blade gives rise to a certain pressure, and between the blades the pressure tends to fall. The fan therefore sets up a rotating pressure-wave which travels in spiral form, and if a microphone is held stationary in space a sound wave is heard. On the other hand if the microphone were rotated with the fan it would be subject to a constant pressure, and no sound would be heard from this particular source. The phenomena discussed above have been examined with the help of the instruments described by the authors, and the various

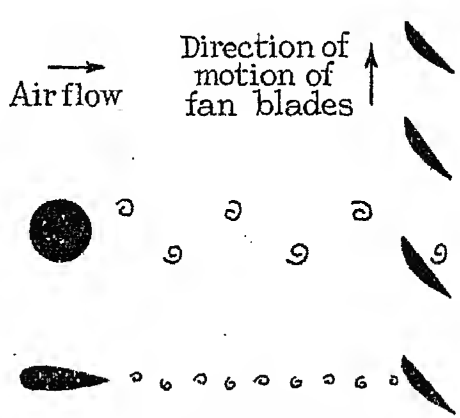


FIG. G.—Effect of vortex stream.

ideas set forward have been fully confirmed by tests. Now that the authors have produced an instrument for the measurement of sound and a satisfactory method of comparing noises, it is possible to obtain information which will be useful in actually preventing noise. The day of the noisy machine is certainly coming to an end.

Mr. A. G. Ellis: The authors define noise as an "undesired or irksome sound." Sometimes music is a noise and sometimes it is not; this presents a practical

difficulty at the outset. For instance, at what time does the continual playing of a trombone cease to be music and become noise? The same applies to the hum of a transformer. The authors say that the hum of a transformer complies with the old physical definition of music. Such a hum may not be a noise in the first few hours of the evening, but it certainly becomes one in the small hours of the morning in a quiet residential district. This illustrates one of the practical problems in the application of noise measurement; the ultimate criterion is the opinion of the observer or the hearer, to whom a measured figure or statement in decibels will be unconvincing if he considers that the sound is irksome. The authors observe that the method of measurement which they have developed is a direct aural method, and it is the sensation experienced by a representative lay listener which is the final criterion. It is not only the magnitude of the noise, however, but also its nature and persistency which react on the hearer. For example, Table 3 shows that the transformers tested have the lowest noise level in decibels in the whole list of machinery, and yet in Table 14 it is rather surprising to find they lie somewhere between a "busy main street" and "trams passing an office on the ground floor with the windows open." In the course of the discussion on Messrs. Churcher and King's paper* in 1929 I remarked that we should await with interest the introduction of a portable sound meter or noise meter which could be taken to a substation and used on the spot. The present marks the epoch when that instrument is actually available. It is very light, portable, and easy to use. Experience has shown that its use can be rapidly learnt and very consistent results quickly obtained as between different untrained observers. My own experience confirms that recorded by the authors, namely that results are obtained by different observers within about 2 decibels. The first difficulty one experiences concerns the comparison of a pure 800-cycle sound with a complex sound made up of noises of various frequencies such as those emitted from a machine under test. In using the instrument it is necessary to regulate the standard 800-cycle sound until it has the same loudness as, and balances, the noise under observation. It is analogous to the early photometers, where a light of any colour was balanced against the yellow light of a candle. A short acquaintance with the instrument soon overcomes any difficulty incident to the difference in nature of the two sounds. With reference to the unit of measurement, the decibel seems to be quite a logical development of the method of comparing sounds, but it is rather a pity that it is not a direct measure of the relative loudness. One of the principal features of the work described in the paper is the experimental co-ordination of loudness sensation with the decibel measurement. The curves in Figs. 5, 6, 7, and 8, give a very good idea of this relationship. Until one thinks in terms of relative loudness one does not get a practical register of the relative degree of loudness of the noise. I have suggested to the authors that if it is not possible to graduate their instrument in relative-loudness units in addition to the decibel scale, a typical curve, similar to those in Figs. 5 to 8 should be attached to each instrument. The subject of noise in transformers has loomed rather large in recent years, principally owing to the widespread use in this country of very large and more or less noisy outdoor transformers; but also on account of the growing practice of installing small transformers in residential districts, outdoors, without kiosks. In the case of the very large transformers there is one aspect which I would impress, namely, that whatever steps are taken to ensure that the noise emitted by the transformer will be the minimum, consistent with commercial and practical considerations, such large transformers operating in the open air are bound to give the impression of emitting a large volume of sound, even though the loudness as measured by the decibel figure at a given point may not be excessive. I think the public have to be educated up to, or down to, this circumstance, and to get used to the idea that the noise of a large transformer is less objectionable (though perhaps more uniform and more persistent) than that of street traffic, which includes such

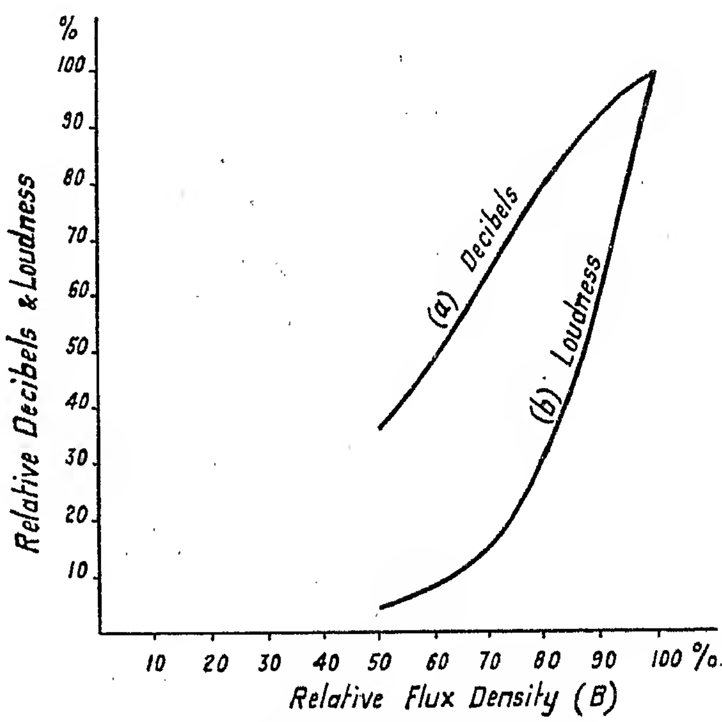


FIG. H.—Curves showing variation of noise emitted by transformer with varying flux density in the core.

noises as motor-cycles with unsilenced exhausts. Table 14 contains some very interesting figures showing quantitatively what the public already tolerates, without usually registering complaints. Turning now to smaller outdoor substations installed in residential districts, there have been cases of complaint by residents, of the noise, especially at night. It is not only the loudness of the noise (which in this case has been regarded as irritating) but also the monotonous persistence of the hum, especially in the quieter hours of the night. The standard commercial distribution transformer is reasonably quiet and is not the subject of legitimate complaint. Complaints come from certain irritable persons, and the noise of the transformer sometimes makes it a scapegoat for what the complainant considers to be the unsightliness of an outdoor substation in close proximity to his sylvan retreat. A good deal can be done by the supply engineer, when choosing and laying out the site, towards ensuring that the transformer is not too near the adjacent houses.

* *Journal I.E.E.*, 1930, vol. 68, p. 97.

The advantage of this, so far as noise is concerned, is well illustrated in Fig. 13, which shows the effect of distance on the loudness observed; for example, increasing the distance from 1 metre to 4 metres more than halves the loudness. If the transformer must be placed close up to residential buildings, probably the best plan is to enclose it in a kiosk or pit, which screens the noise very effectively. Tests on site have shown that the noise observed outside the kiosk is reduced to about one half with the doors closed, as compared with the figure obtained when the doors are open. Investigations have shown that the principal—but not the only—factor influencing the noise emitted by transformers is the value of the induction density in the core. (I am now speaking of transformers of otherwise good design and construction.) The noise has been shown to vary with the flux density as indicated in Fig. H. An appreciable reduction in flux density below the usual commercial value certainly does reduce the noise, but it is impracticable by these means to obtain a perfectly quiet transformer. For example, a reduction of 15 per cent in the flux density has resulted in a reduction of the noise to about one-half. The cost of the transformer is, of course, considerably increased thereby, and it is a matter for the purchaser to judge the economics of the case. Even so, transformers of identical design and construction give different noise figures, and in the present state of the art it is not possible to predetermine accurately and guarantee a noise figure. Investigations into this subject are being pursued and are receiving consideration in the British standardization and research organizations with a view to establishing a standard method of specification, measurement, and guarantee.

Mr. A. T. Chadwick: I am surprised to find that, of the noise sources mentioned in Table 3, the transformer gives the smallest frequency range. It is possible that the measurement of the higher frequencies on the basis of the calculated equivalent-energy method is in general much more accurate than that of the lower frequencies. The authors state, with reference to Table 3, that the difference between the measured and calculated values was not very great in many cases. It would appear that from the point of view of loudness units, however, the difference, even when it is only a matter of 10 decibels in 90, is very great indeed. Much of the difficulty experienced in connection with transformer installations has been the result of the placing of the transformer relative to the surrounding conditions. To place a transformer of 10 000 kVA in the corner of a power-station yard, bounded on both sides by houses within 10 yards of the transformer, is to ask for trouble. The solution of some of the legal and other difficulties arising in that way lies more easily, and more economically, in the hands of the man who chooses the position occupied by the transformer. I appreciate that in many cases there is no choice, and there the transformer must be treated specially. I am strongly of the opinion that this special class of transformer can be, and will have to be, treated very seriously in the future. There are two ways in which this question can be attacked: first of all, from the inside of the tank, and secondly, external to the tank. The first is perhaps the more costly; and a compromise might be the final result. With regard to the treatment of the transformer internally—it is the core which causes the commencement of the nuisance—there is one feature of an examination of the induction density in the core, as compared with the noise emission, which may be of very great importance. If we look at the shape of the loss curve of transformer iron, we notice a fairly straight line up to a point approximating to 9 000 or 10 000 lines per cm^2. I suggest that this point coincides with the point at which the loudness of the hum from the iron commences to become of importance; I should like to know whether the authors have considered this, and whether they have arrived at any conclusion which would suggest this as the induction density at which a quiet transformer will have to be designed. There appears to be a great increase in the rate of reduction of noise immediately below this point. As far as the tank is concerned, there is a far more economical method of reducing the noise emission than surrounding it with a building. First of all, the area to be tackled should be very much smaller, and much less money need be spent upon it to attain the desired amount of attenuation. On page 420 the authors state, " An air space does not in itself cause appreciable attenuation except so far as it constitutes a discontinuity between two walls: hence the thickness of the air space is immaterial." I should like to question this remark. If the two walls constituting the bounds of the air space are very close together, and assuming the air space is closed, the pulsations received on one side of the air space will be received by the other boundary in a ratio dependent on the thickness of the air space, because the air pressure-rise due to the pulsations will be reduced as the air film is widened. This will go on to a point beyond which it would not be thought useful to carry it. I should like to ask the authors the thickness of that air film.

Mr. H. W. Angus: I should be obliged if the authors would state what number of decibels represent an irksome noise. As regards noisy transformers, the magnetic flux is one of the important factors, and the logical assumption is that reducing the flux reduces the noise. The question of the fixing of transformers in suitable positions is very often out of our control. During the last year or two we have erected a number of kiosks in residential areas, and in every case the owners of the land have introduced a clause requiring the Corporation to indemnify the owners of the surrounding property against noise. I have been trying to get transformers that will comply with this requirement, but so far have been unsuccessful in persuading makers to offer transformers with lower flux densities. I hope the work of the authors will lead to the production of a standard means of measuring noise.

Mr. G. A. Cheetham: I have had occasion to use one of the authors' instruments for the measurement of noise on supply meters, and have found that with a noise of the order of 30 decibels two workers—each with a separate instrument and working in different buildings—could obtain agreement within 1 or 2 decibels. I consider that this is an excellent result, especially with a noise of such a small magnitude. A study of the noise emitted by a supply meter has resulted in eliminating one of the causes of noise, which was shown to be itself

producing excessive wear. The study of the problem therefore resulted in an increased life for the meter, which clearly proves the importance of such a study in connection with electrical apparatus. I consider that the decibel is a very unsatisfactory unit; whilst it may be necessary to retain it for some purposes, a scale should be introduced in which the units are proportional to the sensation of noise.

Mr. G. G. L. Preece: There is no doubt that the main solution of the problem of minimizing the noise caused by transformers is lower flux density. Some reduction of the noise can also be obtained by suitably modifying the tank, but if the noise becomes very troublesome the transformer will have to be boxed up.

[The authors' reply to this discussion will be found on page 442.]

Scottish Centre, at Glasgow, 27th February, 1934.

Mr. D. M. Macleod: It seems to me that the methods of measurement suggested by the authors for comparing one noise with another might not be applicable in every case. There is one interesting incident upon which I should like to have the views of the authors. It relates to the curious noise or atmospheric vibration emitted by a blower when in operation. I was at one time associated with the operation of a suction ash-removal plant, and every time this plant started up it caused a curious vibration which particularly affected a certain house situated about a quarter of a mile away. The window sashes vibrated and the china arranged on the shelves of the kitchen rattled, to the annoyance and discomfort of those living in the house. It would be interesting to know whether the authors' methods could have been applied in such a case, to detect the cause and suggest a remedy. The trouble was got over in rather a curious way. The discharge from the blower was ultimately blanketed as a result of the extension of the coal reserve at the works. The result was that the nuisance was completely stopped, and even the atmospheric vibrations were no longer noticed.

Dr. M. G. Say: Presumably the condenser microphone was calibrated for lower frequencies by a method using resonating tubes, and for higher frequencies by means of the Rayleigh disc in an enclosure lined with sound-absorbent material. Is there adequate agreement between the results obtained by these methods in the overlapping range of intermediate frequencies? If the authors would give a brief description of these test methods, or a bibliographical reference, it would greatly assist me in the task of initiating a course of regular laboratory sound-measurement work at the Heriot-Watt College. A drawing of the microphone, which is described as having a flat face, would be valuable: those that I have seen have had recessed faces. The rise in threshold shown in Fig. 1 at a frequency of about 1 600 cycles per sec. seems unusual. Is it connected with the method of placing the subject when making observations? The wavelength of sound in air at this frequency is of the order of 8 in., or comparable with the size of the head. Possibly the disturbance of the wave-front produced may in some way account for the unexpected departure from smooth curvature of Fig. 1. Audiometric measurements recall the definitions of electric and magnetic fields in terms of unit charges and poles, which are presumed to be so small as not to disturb the fields they are called upon to measure. In audiometry the ear (with its associated head) can hardly be held to have no influence on the sound field, thus introducing considerable technical difficulty. The test under discussion was performed by measurement of the excess pressure by microphone at the position of the subject, but in his absence, so that the conditions of the two observations were unavoidably different. Although in audiometry the final reference standard is the ear, I think that statistical averages form an invaluable aid to the engineering aspect, which has to deal with mass effect, whether of noise in general or of sound reproduction for public address systems, radio reception, or sound-film operation. The relation

$$\text{Loudness} = \text{antilog}\ (\text{Decibels}/50)$$

should be of considerable use in these activities, as it relates in a simple fashion the usual decibel measurements to their effective physiological value.

Mr. A. B. Eason: I am especially interested in the paper in view of the problems I have had to deal with in getting rid of noises transmitted in buildings from distant rooms. In the first case there were two 12-h.p. motor-generators and a small $\frac{1}{4}$-h.p. telephone ringing machine; we stopped the motor-generators, but the people in the room below still complained of the noise. We found that the troublesome noise was due to the $\frac{1}{4}$-h.p. motor, and were able to eliminate all the noise quite simply by putting a very flexible support underneath each of the machines. Two or three cases arose in Glasgow where the noise from machines was troubling tenants below; in each case we used the same remedy, namely a very flexible support for the machine. In a recent case where a motor-generator caused considerable noise in a test room and interfered with telephone work, the nuisance was removed by placing round and over the machine a wooden case lined with shavings and sacking on the inside, to absorb the noise; although plenty of space was left for the air to reach the machine, and openings were left in the top for warm air to escape, the noise was reduced sufficiently to allow work to proceed normally in the room. It would be interesting to take one of the instruments mentioned in the paper into the Glasgow Stock Exchange in order to measure the noise level when the market is in full swing, for the noise level is certainly much higher than is usual where people wish to telephone. The problem of supplying suitable telephones is an interesting one, for if the transmitter is very efficient, so that the user's speech reaches the distant speaker well, it will also transmit the general noise efficiently; whereas if somewhat less efficient transmitters are used the speech at the far end may not be so good, but the noise is absent.

Mr. A. P. Robertson: The practical aspect of noise emanating from electrical or mechanical machinery is not easy to define or to measure by means of instruments. There are two results of noise from machines, one physical and the other mental. A noise may be a nuisance. It

is not necessarily a physical evil, but it may result in physical evils in consequence of the mental attitude to it. It is very often found that a noise of which the pitch is low has more physical effect than a high note, which has greater mental effect. The noise which comes from a transformer is generally of a rather low pitch and has definite physical effects. Low-pitched noises can be reduced very materially by placing the machine on a resilient mounting; it is more difficult to get rid of a high, screeching noise. In one case of a complaint about the noise emitted by a transformer I traced the cause to a bar supporting a cable passing through three or four floors of the building. When the bars were cut and the cable supported from the floor, the trouble ceased. When dealing with screeching noises the aural method of distinguishing is much better than the use of an instrument. I should like to ask why 800 cycles per sec. is taken as the basis, and whether this note is a musical or a noisy one.

Mr. P. H. R. Durand: The authors showed a slide illustrating a small motor on the test block in the listening-room connected by a shaft to a generator in another room, the two rooms being sound-insulated. Is there not a possibility, even with a flexible coupling, of the results being somewhat inaccurate on account of the transmission of extraneous noises through the coupling and shafting into the listening-room?

Mr. C. Penn Hughes: The firm of transformer manufacturers with which I am associated have been asked recently by the Central Electricity Board to mount certain transformers on anti-vibration pads. I should like to know whether the placing of this apparatus on anti-vibration pads will affect the volume of noise. I should also like to know the relation between amplitude and frequency, and whether by using anti-vibration pads or padded walls one can get rid of transformer vibration.

Prof. S. Parker Smith: The authors have not tried to convince us that there is anything final in their work; they have rather put forward a method that can be used until something of a more scientific nature is evolved. One troublesome matter is the composition of the noise. Noises or sounds may not seem the same to all people; indeed, to judge from modern music it is probable that something which is pleasant to one ear is unpleasant to another. Experience shows that whether noise is a nuisance is not altogether a matter of loudness. With regard to the measurement of noise, have the authors tried to develop the cathode-ray tube for this purpose? The cathode ray is extremely sensitive, though the difficulty might be to calibrate it in terms of loudness. I should like to know whether the authors have investigated the distribution of the sound in the neighbourhood of a machine. As is well known, this may vary greatly owing to interference. Does this factor affect their results?

[The authors' reply to this discussion will be found on page 442.]

South Midland Centre, at Birmingham, 19th March, 1934.

Dr. M. L. Kahn: The authors have attempted to give us a new unit—the unit of sound—and, still more important, have produced a balance which will measure sound in the same way as the chemical balance measures mass. The importance of the fact that we can now measure the intensity of noise, is that it will enable the B.S.I. to work out a standard specification for the noise which is permissible for various classes and various applications of electrical machinery and apparatus. A customer will then be able to buy a machine to a specified noise-limit, just as he can do so now to a specified limit of temperature-rise. The Post Office have already issued a specification dealing with noise, but this refers to electrical noise, which only appears as telephone disturbance. They have also developed an apparatus, based on somewhat similar principles to those of the authors' sound balance, which can compare noises produced by currents in a telephone. I should like to ask the authors a few questions. In the first place, when a specification for noise is issued or settled by the B.S.I. we shall have to have some limit similar, for example, to the temperature-rise limits depending on general conditions of 40 to 50 deg. C., or higher limits, which depend on the insulating materials used. For instance, a machine or transformer which is installed in an engineering shop need not be as quiet as machinery placed in inhabited buildings, e.g. for working lifts. How far has the work progressed towards determining these limits? Secondly, I have found that, apart from the absolute value of the noise, so far as the effect of the noise on the human ear is concerned the locality of the apparatus which has to be investigated is very important. It has been found that lift motors which seem satisfactory in "silent" rooms are noisy in practice. This phenomenon has been traced to the resonant effect of the lift shaft. What has been the authors' experience in this connection? Thirdly, there are three kinds of noise caused by electrical machinery: (*a*) magnetic noise; (*b*) mechanical noise, due, for example, to the whistling of brushes or the rumbling of ball bearings; (*c*) ventilation noise. I find that magnetic noises penetrate much farther than the other two. This may be due to the high frequency. Have the authors made any experiments to throw light on this? Finally, how far have they been able to separate the external noises from the noise of the machine itself? This point will have to be specially considered in connection with a specification to be issued by the B.S.I., as it would be quite impossible to test large machines in a silent chamber.

Mr. A. G. Engholm: I had occasion only to-day to visit a works in order to investigate some fans in unit heaters. Some of them were giving out greater amounts of noise than others. The fans were suspended from girders, some being situated quite close to the corrugated-iron walls of the workshop and others against a wooden partition. Much less noise was emitted from those next to the wooden partition than from those close to the corrugated-iron walls. To take a simple example, that of a watch ticking, if the watch is placed on a table with a glass top one can hear it ticking quite loudly, yet when it is placed on a bed one cannot hear it at all. How is this kind of sound to be measured? The means of

mounting has clearly a very important effect on the sound produced.

Mr. C. R. Woodward: I should like to know why the authors have worked within the frequency limits of 100 to 6 400 cycles per sec., seeing that most of us are able to hear sounds of both higher and lower frequencies than are defined by these limits. A curve similar to that of Fig. 1, but inverted, represents the threshold of pain, and one can therefore see that noises at the ends of the audible range need not be very loud to cause a nuisance; I find that a quiet noise of about 10 000 cycles per sec. is most irritating. I consider that the authors should have extended their measurements to both higher and lower frequencies, so as to include those sounds which are more easily transmitted through structures (low frequencies) and the noises of high-speed machinery, hissing steam, and the harmonics thereof. Here is a simple method of measuring approximately the noise level of a situation, described by Dr. A. H. Davis of the National Physical Laboratory. Strike a tuning fork in a very quiet place, hold it close to the ear, and note the time in seconds from the moment of striking to the moment the note dies away; do the same in the presence of the loudest noise it is possible to find or produce, and assume that it is 100 decibels louder. The difference of the two times can be converted into an arbitrary scale of loudness, and so one can estimate approximately the loudness of any offending noise. I agree with the authors' remarks about the inconsistency of the decibel, and consider that many of us will have to use their "loudness units." I am a little sceptical about their methods of measuring sounds; they seem to get consistent results in spite of the fact that there is a definite masking effect of one sound in the presence of another of different intensity, pitch, and wave-form. This effect is due, I presume, to the non-linear characteristics of the ear and the phase relations of the two sounds.

Mr. C. G. Mayo: I am inclined to think that this paper marks the opening of a new era in engineering. Hitherto engineers have confined themselves to measurement, leaving the difficult ground of ultimate values to others. The authors, however, have the courage to approach the subject from the point of view of human values; and, since everything the engineer makes is ultimately for human appreciation, this represents a return to fundamentals. The loudness and annoying power of noise are essentially subjective, and the authors by eliminating impersonal measurements have avoided a morass of difficulties. The paper deals very satisfactorily with commercial measurements devised to demonstrate that a machine is quiet. Equally important for the designer, however, are measurements to determine what frequencies are present, and at what intensities. At low frequencies it is difficult to obtain this information by means of the microphone, since large radiating surfaces are required to transmit low frequencies across air, and such vibrations are chiefly transmitted through the solid foundations. It seems to me that what should be measured is the movement of the motor frame, detected mechanically by some device such as the gramophone pick-up. This would give information as to the actual source of the noise, independent of the mounting. I should like to know whether the authors have done any work on these lines.

Mr. E. G. Ross: I have had some experience of gear-box testing by means of a needle-driven microphone instrument, the needle being attached to the wall of the box. The instrument, of reputable make, was scaled in decibels. When driven while using one form of lubricant, the box emitted quite a pleasing whine, but when the lubricant was changed it produced an excruciating howl. The strange thing was that the instrument read much less pressure when the box was howling than when it was whining. The authors do not refer to timbre, which forms one of the outstanding difficulties in the making of noise measurements. In Table 14, three items are given as having a noise value of about 100, namely, the steel-tank shop (100), a pump house (102), and a machine room (100). I should much prefer to live in the pump house rather than the tank shop, while the machine room is even better. The authors stress the liability to error of observation when dealing with harmonic sounds, or sounds having a large proportion of harmonic vibration in them. I suggest that this error is caused by the influence of timbre, as these examples witness. By merely changing the metal of gearwheels in a box, while having them machined to the same limits and driven under exactly similar conditions, the noise and timbre have been reduced to a level which can be regarded as reasonably pleasant. I should like to ask the authors to amplify their remarks so as to afford details of any experience they have had in attempting to assess or measure timbre as distinct from pressure.

Mr. C. Stokes: I gather from the authors' remarks that in the case of a 50-cycle transformer 100 cycles per sec. was obviously the fundamental frequency of the noise. This does not seem very obvious to me, and I should imagine that the unsymmetrical response of the ear would cause the presence of a 50-cycle component, which should be regarded as the fundamental frequency. A few days ago I had the opportunity of trying out a mechanically coupled microphone, and I found great difficulty in keeping this truly stationary. The source of vibrations, although not very powerful, was sufficient to cause considerable movement in a fairly heavy surface plate, and substantial discrepancies in the readings were obtained. The most consistent results were obtained by holding the microphone in the hand.

Mr. Emlyn Jones: I should like to ask whether the authors have found it necessary, when making measurements in an enclosed space, to take into account the effects of standing waves due to interference between the direct and reflected sound-waves. These effects produce great changes in the sound intensity from point to point, particularly when, as in the case of electrical machinery, the noise contains a comparatively small number of components, one of which is usually very prominent. This would, of course, only occur in a room having highly reflective surfaces, but such surfaces would often be encountered in practice. The difficulty could probably be overcome by making a series of measurements from different positions, and I should like to know whether such a procedure has been found necessary in practice.

[The authors' reply to this discussion will be found on page 442.]

East Midland Sub-Centre, at Leicester, 20th March, 1934.

Mr. J. H. R. Nixon: It would be particularly interesting to know of practical results which have been obtained by applying the authors' methods of measurement with a view to suppressing machinery noises. The chief types of noise emitted by electrical machines are the noises due to windage (i.e. air noise), rubbing noises (from bearings and brushes), noise that has its seat in magnetic vibrations, and the vibratory noise due to mechanical causes. It appears to me that the authors' system of measurement could be applied as a means of isolating different noises. Has research of this sort been carried out on machinery, and, if so, with what results?

Mr. A. Brookes: The authors' definition of noise (page 401) may be simplified to "undesired sound," because the wanted noise may be irksome whereas the unwanted sound may be music. For example, the overhearing of music transmission from a B.B.C. landline on a telephone call is disturbing and represents noise. If loud enough it may spoil the desired speech transmission. The method of arriving at the threshold value given on page 402 is certainly suited to objective methods, but in relation to subjective methods it leaves something to be desired. Bone conduction does not appear to have been fully considered, although it must have been present in all the subjects to a greater or less degree, in the bones both of the head and of the hand and wrist. This would certainly give rise to errors if a telephone receiver were used against the ear for subjective measurement, as the readings obtained would not take account of bone conduction. Turning to page 407, I am surprised that no difference could be found due to the use of one or two telephones held to the ears. This was evidently due to the experiments being conducted in a soundproof room. In an open space, even if relatively quiet, much greater sensitiveness is obtained with a telephone on each ear than with a single telephone, even if current is passing in one of them only. I can understand that isolated changes are more easily and more accurately detectable than cyclic changes, and that the increasing-intensity results are more reliable than the decreasing ones owing to the effects of retention of impression. It is very difficult to get reliable data from personal observations in regard to stimulus, and, despite the authors' statement that their method is an improvement on the decibel scale, it must be admitted that the latter gives something definite and obtainable by physical measurements. Confusion appears to be general in respect to the significance of the decibel; although it has a physical meaning it is simply a mathematical term. Our experience in regard to the determination of the proportion of loudness intensity does not agree with the authors'. We use a balance method if possible, with two telephones placed to the same ear alternately (not one on each ear), and find that balance can be obtained to a surprising degree of accuracy for either speech or tones. Estimations of loudness are not used. The obvious difficulty is in the fixing of the comparison standard; but for telephone measurements we generally express our results in terms of a definitely calibrated standard. I agree with the authors' statement on page 418 regarding the accuracy of aural balances, particularly with skilled observers. For speech tests we eliminate bias by the use of secret attenuation inserted by another observer. Referring to page 408, the authors' claims regarding estimation of sound level are surprising. In our experience quite expert observers have not been able to do as stated, within a considerable margin. Turning to page 419, is the decibel scale a logarithmic scale of stimulus as quoted? Regarding Tables 3 and 5, it seems very surprising that the differences between the calculated and measured values are greater the simpler the noise, i.e. the less the number of components and the more restricted the frequency range. One would have thought that the transformer quoted would have given the closest results, not the reverse. I note that the discrepancy is greater when the components are harmonically related. I should be glad if my above analysis could be confirmed by the authors. It would almost appear that the addition of frequencies not in harmonic relationship might reduce the apparent loudness. In speech transmission the addition of unwanted frequencies produces a marked effect upon the articulation or intelligibility of the speech. The variation in sensitivity caused by warming the telephone (page 418) is most probably due to the change in the magnetic air-gap dimension, not to changes in mechanical properties as suggested by the authors. Turning to page 421, if two sounds are present forming a complex sound, surely they must be taken as a whole and not analysed separately. It is not possible to measure one sound in the presence of any other, on account of masking effects, unless, of course, an elaborate harmonic analyser is used, and if this is done the method is objective, not subjective.

Mr. J. Morris: Regarding the question of subjective observation ("measurement" hardly seems the correct term to use in this connection), it appears that insufficient attention has been devoted to the physiological aspect. According to the authors the sound measured or observed is the total stimulus from all sources, including bone conduction, but there appears to have been no attempt to analyse the reactions of the various observers in the test outlined. Human beings are affected in many ways by vibrations of widely differing frequencies. Some individuals are adversely affected in the presence of a powerful organ when the lowest notes (32 cycles per sec. and thereabouts) are being played, the discomfort in some cases approaching actual physical pain. The conveyance of the vibrations is not in all cases done by means of the ear; it is sometimes effected by the actual sensation of feeling. Clearly the measurement of this form of stimulus is extremely difficult. I should like to mention an experience I had a few years ago in connection with certain experiments with apparatus for the measurement of deafness, a problem closely linked with the one under discussion. During one test I was shut up in a very quiet room, with heavily carpeted floor and drapery-hung walls; reception was carried out on a shunted-telephone type of audiometer, and when I placed the receiver down prior to making adjustment I found I could still distinguish, faintly but clearly, the

transmitted sounds. Although the apparatus was disconnected, spoken words were still distinguishable. About 80 per cent of the words were received correctly when spoken in a voice slightly above a whisper in an outer carpeted and ordinarily furnished room. Tests made on other individuals met with little success, the results being practically negative in each case, although every care was taken to ensure exactly similar conditions. The possibility of the walls, door, etc., acting as a sound radiator was considered, but even if they had acted in this way it would have been impossible to explain why one person could distinguish sounds which apparently could not be detected by others, all persons of normal hearing. This experience suggests that where absolute, or even comparative, measurements are to be carried out in a way which will carry conviction to an interested person, subjective methods must in the end give way to the objective type. Such methods are at present, however, a good deal more expensive. Microphones of great fidelity of response, and amplifiers to cover a very wide range of frequencies, are being devised and simplified to a great extent. The use of direct-reading logarithmic voltmeters of the type recently described by F. V. Hunt,* which utilizes the logarithmic relationship between the grid and anode voltages in certain types of variable-mu valves, should lead to the production of an objective type of noise meter upon which those who are not experts in noise measurement will be able to place reliance. In conclusion I would express the opinion that closer collaboration between the engineer and the aurist might result in a more rapid appreciation of the physiological aspect of noise measurement, and lead to a solution of the problem of how we hear.

Mr. A. C. Hutchinson: Analysis of the authors' scale of loudness values reveals that the loudness of a simple sound varies roughly as the fourth root of its sound energy. This approximation is of immense assistance in practical reasoning about problems of noise, and may be employed to justify two important principles of noise reduction, (*a*) that all the noise-emitting surfaces of a machine must be located and covered, and (*b*) that aural estimates of effects should be made at close quarters. It can be deduced from the law quoted above that the loudness of a sound should vary inversely as the square root of its distance from a listening point, and I have employed this relation for making rough measurements of machine noises by comparing them with a standard noise, in this case a 4-volt miniature horn. A listening point having been chosen at a standard distance from the machine under consideration, the standard noise was moved towards or away from that point until it appeared as loud as the machine's noise, when its distance from the listening point was noted. I should like to suggest that the next most important step towards the solution of engineering noise problems is the enunciation of some law—however approximate—to connect the noise levels due to a given source in free and enclosed spaces, taking into account the dimensions of the space, the nature of its enclosing surfaces, and a sort of "mean spherical loudness" quantity for the emissive power of the source. [The remainder of Mr. Hutchinson's remarks was similar to his contribution to the London discussion (see page 427).]

Mr. F. V. Pipe: There is little doubt that when some reliable means of noise measurement is available the intensity of noise to be emitted by a particular machine will become a clause in the acceptance specification. At the present time the question of noise becomes a matter of opinion between the customer's inspector and the manufacturer. In the production of machines for special purposes where the elimination of noise is of particular importance, e.g. machines for installation in telephone exchanges or for the operation of telephone apparatus, it is necessary to take into account both mechanical noises and electrical noises. Commutator ripple, which may be transmitted to the telephones, is an example of the latter type of noise. Here it is obviously necessary to have some sort of standard by which to decide the acceptance or rejection of the machine. Some noises can be reduced by the expenditure of more time, trouble, money, and material, while others cannot be reduced below a certain minimum; consequently there is an incentive to produce a reasonably good machine at the lowest possible cost, whereas it might be possible to produce a rather better machine at a slightly greater cost. Some form of standard is necessary which will protect the manufacturer from the competitor who offers a worse machine from the noise point of view at a lower price, and will also ensure the user satisfactory operation in service. We have found that in measuring noises by the aural-comparison method it is comparatively easy to strike a balance between two pure noises of about the same frequency, but it is by no means easy to measure a noise made up of mixed frequencies whose general frequency is very much lower than that of the standard pure note.

Mr. F. G. Macdonald: There is no doubt that if we could "see" sound we should be able to study the subject of noise much more easily. I think there is an instrument, called the "harmonograph," which enables us to see sound. Surely we could obtain "graphical sound" by means of such apparatus, and thus be able to find out a lot more about sound than we can at present. I should like to know whether the authors have used a harmonograph for detecting sound.

Mr. W. N. Bray: In the first part of the paper the authors are trying to arrive at a straight-line relation between stimulus and sensation. They refer early on to the fact that, in accordance with the Weber–Fechner law, a scale for the interpretation of the r.m.s. pressures of pure tones in terms of the proposed units of sensation was evolved. Later, in Fig. 7, they arrive at a curve of decibels and on taking antilogarithms obtain a straight-line relation. There seems here to be some redundance, and I should like to have the authors' views on this point. They mention two circular saws providing noises of 160 decibels, and compare them with one circular saw and the ticking of a watch. If 160 watches were put in a box, however, they would not make as much noise as one circular saw. To obtain equality between the two noise levels, would the watches have to be so synchronized that they all ticked at the same moment?

[The authors' reply to this discussion will be found on page 442.]

* *Review of Scientific Instruments*, 1933, vol. 4, p. 672.

NORTH-EASTERN CENTRE, AT NEWCASTLE, 26TH MARCH, 1934.

Mr. A. T. Robertson: Noise is becoming a greater nuisance the more we become industrialized, and a standard method of measurement of this nuisance on an agreed scale is obviously essential. It must be admitted that the decibel scale is inadequate for indicating subjective loudness, and since it is the sensation which the noise causes an individual that is the nuisance, a scale that bears a fixed relation to sensation rather than stimulus seems to be required. The authors have shown that a logarithmic scale of physical stimulus does not correspond to an arithmetic scale of sensation; the relation between the two is, in fact, so far from one of direct proportion that it is doubtful whether a scale of decibels above threshold is of any real value for lay use. For most investigations involved in the reduction of noise, frequency-analysis investigations using a physical scale are of course essential. Some 25 years ago I used the subjective balance method A (page 416) for the determination of the forces on the cutting tools of lathes, and found the results obtained by a number of observers to be remarkably consistent. This was to be expected, because the noise and the reference tone were identical when balanced. I am surprised at the closeness to which various observers agree in their estimates of half and quarter loudness, but the authors seem to have taken every precaution to ensure that the agreement is not fictitious. Having determined a scale from zero (threshold) to 100 decibels above threshold, the authors suggest that the most trustworthy method of determining the loudness of a noise on this scale is the aural comparison method. American investigators* claim, however, that the "total noise" meter will give more consistent results than any single observer, and will give similar results to the average of a sufficient number of observers to eliminate individual discrepancies. When the noise is of a nature that is not disagreeable, its deafening effect rather than its loudness is a measure of its nuisance, and for this reason the masking method of assessment would seem to be the correct one to employ, rather than that of aural balance. It is suggested that the increased loudness for a given stimulus experienced with harmonically-related noises, such as those obtained from transformers, is due to the hearing system producing a sensation due to the sum and difference tones: can this suggestion be confirmed by calculation?

Mr. W. D. Horsley: Section (2) of the paper, which deals with methods of re-determining the properties of the hearing system of the average individual, is interesting and the results obtained appear to be consistent. A considerable amount of research work still remains to be carried out, as is indicated by the experiments described in Section (4). These experiments show that the noise due, for example, to a transformer, which consists of a fundamental note and harmonics of the fundamental only, has a higher sensation level than a noise of equivalent energy which contains components of various unrelated frequencies. Had it not been for this peculiarity of the human ear, a satisfactory noise-measuring instrument of the objective type, i.e. "an artificial ear" as defined by the authors, could no doubt have been readily developed. In order to meet the demand for a noise-measuring instrument which will enable various types of apparatus to be compared on a reliable basis, the authors have resorted to the subjective method and have produced an instrument which gives satisfactory results in practice. The measurement obtained does, however, depend to some extent upon the observer, and it may be necessary for several tests to be made by a number of persons. For this reason, the objective method, in which a direct reading can be obtained from the noise-measuring instrument calibrated in suitable noise units, is much to be preferred. The investigation should therefore be continued until an instrument can be made to simulate the response of the average human ear, taking into account all of its peculiarities. With regard to Section (5), a large number of noise problems arising in engineering and other spheres are not due to noise emanating directly from the apparatus which is the source of the energy, but are caused by resonance or by the transmission of vibrations through floors or along steelwork. The apparatus may be well within the required standard of noise level, as measured by the noise instrument, but the amount of energy required to produce a loud sound is so small that a slight vibration—if transmitted to a suitable sounding surface—will cause an unbearable noise. An interesting example of this kind of problem is that of a geared turbo-alternator set which was installed in a power station at the lower end of a long street. At the upper end of the street was situated a bowling green, which was above the street level, and to some extent closed the street at that end. An unpleasant drumming noise was experienced in the bowling green when the set was running. It was found, on walking down the street in the direction of the power station, that the noise disappeared and then reappeared again for a short distance about one-quarter of the length of the street from the lower end. In the power station, the noise level was normal for this type of plant and the predominant note was that of the gearing. It was eventually found that the cause was air vibrations set up by the coil connections on the alternator rotor. The street was evidently acting as an organ pipe whose natural frequency corresponded to that of the air vibrations from the rotor. A very slight modification to the connections cured the difficulty. In another instance excessive noise was experienced in an office adjoining a power station. It was found that the girders supporting the floor extended into the engine room and rested upon one of the foundation blocks. Vibrations were apparently transmitted along the girders, and an objectionable noise was built up in the office owing to resonance. The difficulty was overcome by severing the direct connection between the girders and the foundation block.

Mr. C. Giles: The decibel scale, of which I believe the authors were the originators, is most unsatisfactory to use in practice, as it has not a straight-line connection

* P. H. GEIGER and E. J. ABBOTT: "Sound Measurements versus Observers' Judgments of Loudness," *Electrical Engineering*, 1933, vol. 52, p. 809.

with loudness as perceived by the ear. Readings in decibels therefore convey no meaning as to relative loudness to the average engineer, and even those with a good knowledge of the scale find decibel readings difficult to interpret. The ideal is a scale like most other scales, in which 50 units of noise are $\frac{1}{2}$ of 100 units, and 25 units are $\frac{1}{2}$ of 50 units; not 1/20th and 1/5th respectively. Such a scale is shown in Fig. 7. It would be a great help to the better understanding of relative noise-levels if such a loudness scale could be universally adopted instead of the decibel scale for use in noise-measuring instruments. Although the curves shown in Fig. 7 were obtained experimentally by aural tests on 34 people, I have found that they can be represented very closely by the expression: Loudness $=$ (Decibels)$^4 \times 10^{-6}$; they would therefore appear to have a sound physical foundation. I am sorry to see that the authors have not attempted to give, as a standard, some recommendation as to how far from the source of noise and at what height above the ground the noise should be measured. The figures given in Table 3 are of little value for this very reason. I should like to know whether the concrete foundation blocks shown in one of the slides are set firmly into the ground or rest on some sound-absorbing material.

Mr. E. Fawssett: I am reminded of my Address to the Meter and Instrument Section,* in which I suggested that there was no accepted definition of the difference between "music" and "noise." I rather feared I was in error, but notice that the authors seem in doubt too, and have had to frame their own definition. Personally I would invert their illustration of modern orchestral compositions, and say that people with an appreciation of the works of Beethoven, Mendelssohn, and the like, would certainly classify the modern compositions as noise; all of which shows my original contention, that there is no recognized dividing line, to be correct. I note in Fig. 1 that the authors have only been able to go up to 6 400 cycles per sec., and that the curve only applies to the weak sounds at the threshold of hearing. I should like to know whether this curve applies in general to much louder sounds—say, of the order of 80–100 decibels—and, if so, whether if the total amounts of noise emitted by a machine at 4 000 and 8 000 cycles per sec. respectively were the same, the sensation to the human ear would be very much less in the latter case. I rather doubt the suitability of 800 cycles per sec. as a reference for all sorts of complex noises. It seems to me rather on a par with trying to balance the intensity of a red light against that of a green, which is not as easy as comparing both against a white. The white light, being a mixture of colours, compares with a complex noise, which seems to me the better standard. Then I would prefer to use one ear only and an offset receiver, employing the masking method, which to untrained ears seems to give very satisfactory and consistent results as between different observers. There is another feature of noise which has not been referred to, that is whether it is intermittent, and, even worse, irregularly intermittent, or continuous. I am reminded of a case of an a.c. lift motor starting up, the noise from which in a nearby examination room rendered work impossible largely because of its intermittence. The same noise, continuously audible, would not have been insupportable. The annoyance due to a noise is also greatly dependent on the background. An electricity meter may develop a slight buzz or hum of the order of 20 decibels above zero, which is absolutely innocuous in ordinary positions in daytime where the general level may be 30 decibels or more: but at night, if the meter is fixed outside a bedroom door, the noise may become utterly unbearable. We have had many meters to change from this cause; the noise emitted from them was almost impossible to detect in the test room. Other domestic electric noises that are a source of nuisance are the buzz caused by a radiator owing to some metalwork getting into step and emitting a noise of about 30 decibels 5 yards away in a quiet room, and, worst offender of all, the noise due to a vacuum cleaner. The latter can be heard all over a house, and in another room has a volume of the order of 50 decibels; in the same room up to 70 decibels or more. The decibel scale has been severely criticized: for the general public it is not very suitable, as it is not easy for them to realize that 100 decibels represents a noise of much greater intensity than twice 50; but to the engineer who can train himself to think logarithmically, the scientific basis of the unit should appeal.

* *Journal I.E.E.*, 1931, vol. 69, p. 25

Mr. J. A. Harle: The paper presents a very important advance in the subject of noise measurements, as its strong practical bias distinguishes it from papers dealing with the purely physical aspect of sound. The object of the authors' work, the suppression of all mechanical noises, is becoming more important as machinery enters into modern life. It is obvious that the apparatus necessary for the carrying-out of this work must of necessity be of two kinds: the first that dealing with the assessment of the total noise, and the second that dealing with the analysis of the noise, which is of primary interest to the persons responsible for locating and eliminating the source of the noise. I think the scheme proposed by the authors for assessing the total noise is on the right lines as the apparatus is portable and can be applied anywhere, and their test results show that consistent readings can be obtained from a variety of operators. In addition to the work carried out on the purely technical side of sound measurement, there is considerable scope for work on the physiological side, i.e. the determination of what is the maximum background noise that average people can tolerate before it becomes a strain on the nervous system. The modern vogue of large offices containing large groups of people, for example, tends to increase the intensity of background noises; and fans, ventilating systems, etc., also add their quota to this factor. In an office with which I am associated we frequently find that if the rate of working of the combined heating and ventilating system is suddenly cut down, the staff at once realize that they are shouting. Such conditions undoubtedly add to the physical strain of those working under them. There is no question that legislation will ultimately have to be introduced to deal both with street noises and with the noise emitted from adjacent buildings, and the measuring device proposed by the authors should have its field in assessing such noise. Broadcasting has introduced

problems with regard to noises from adjacent buildings, and I think it is time that legislation was passed dealing with the question of how much noise should be allowed to be transmitted from one occupier's premises to another. The people listening to the broadcast programme would describe the noise as music, but it would appear that noise can only be music if one wants to hear it. It is up to the electrical industry, which has created broadcasting for the world, to ensure that the people occupying adjacent buildings are protected against careless operation of the receiving equipment. I think it very desirable that the administration of such anti-noise legislation should not be placed in the hands of local authorities, because tramcar systems are generally among the greatest offenders from the point of view of noise production, and the usual attitude of such authorities is that noise is objectionable if it is produced by apparatus in the possession of private persons but not if it is emitted by apparatus belonging to the local authorities.

Mr. J. F. M. Mellor: Just as the contraction of the iris protects the human eye from a brilliant light, so apparently some agent de-sensitizes the ear to prevent discomfort from sounds of considerable intensity. An observer, concentrating on considering the main component of a noise, might have his ear de-sensitized by an overtone of that note, which, although of lower intensity when measured in dynes per cm^2, might stimulate the ear to a greater extent than the lower note, as estimated from the frequency/sensitivity curve of the ear relating pressure and frequency. This effect would influence measurements of sound intensity made with an apparatus employing the human ear as a comparative operator. Is this effect taken into account when assessing loudness with the "artificial ear" mentioned in the paper? I should like to know whether the relation between the sensitivity of the ear and various audible frequencies follows the threshold-of-hearing curve (Fig. 1). The absence of uncomfortable aural stimulation, when one is listening to the low note of a large organ pipe or to the high-pitched noise of escaping steam, seems to indicate that hearing is most acute for loud sounds of frequencies at which it is most sensitive on the threshold of audibility. This means that the sensitivity curve is inverted for vibrations of great intensity. To express the noise disturbance of a machine should we not use a scale for assessing the noise intensity from the machine above the normal noise of that locality? A transformer hum of intensity 60 decibels above threshold might not worry men working in a factory beside the apparatus during working hours, but that hum intensity, at the same distance from the transformer in a rural substation, might be very annoying to people living close to the troublesome apparatus.

Mr. W. F. Smith: The authors' threshold calibration measurements have been made on pure tones, and for the complex tones it is assumed that a note of a certain frequency and loudness will give the same sensation no matter whether it is heard as a pure tone or as one component of a complex sound. This assumption cannot be avoided, but in making the threshold measurements great care has been taken to avoid background noise, which has only the same effect (i.e. de-sensitizing of the ear) on the pure tone as the presence of other frequencies might have. It would seem, therefore, that the presence of some little background noise may even lead to a result more in keeping with actual conditions. Again, certain frequencies may have a greater de-sensitizing effect on some persons than others, and the threshold of hearing of a pure tone when incorporated in a complex sound may vary quite apart from the variation due to the different aural qualities of individual observers. With all these limitations on the measurements made by the subjective method, the objections to the use of an "artificial ear" tend to relative unimportance on comparison. If the artificial ear were made to cater for the main components of a complex sound it should be possible to compare sounds reasonably accurately as far as industrial engineering noises are concerned, even if not for communication engineering. The use of an artificial ear is a scientific method, and although absolute measurements may not be possible it is thought that comparative readings would be more reliable than those of the subjective method. The authors have accepted the subjective method on the agreement shown by the observations of only 4 observers in some cases, but, owing to the wide differences in the aural senses of individuals, I consider that the observations of about 400 persons are necessary before the accuracy of the method can be assumed. In making comparison between a noise and a standard of reference a method is employed whereby the standard is applied to one ear and the noise to the other. The responses of both ears to any particular sound are hardly likely to be identical, and even the average of tests made by interchanging the sources of sound is likely to give rise to errors owing to the fact that the response of the ears varies so much with frequency.

The Authors' Reply to the Discussions at London, Liverpool, Manchester, Glasgow, Birmingham, Leicester, and Newcastle.

Messrs. B. G. Churcher, A. J. King, and H. Davies (*in reply*): The discussion has brought to light the great differences of opinion which are to be encountered on the question of noise and the determination and statement of its magnitude among engineers and acoustic technicians. To a large extent the divergence is due to the failure by some people to appreciate the fundamental point that noise is concerned at least as much with the response of the nervous system as with the magnitude and frequency of air vibrations. This being so, it is inevitable that noise cannot be stated completely in terms of physical units, but that attention must be given to the magnitude of the sensation experienced by the individual. Since there is no known way of measuring a loudness sensation directly, the method we have used, that of determining the magnitude of a suitable standard stimulus which evokes in average people equal loudness sensations, seems to be the only one available. It

follows, therefore, that whatever other methods of assessing loudness are proposed they must be calibrated or checked for the type of noise in question in terms of this standard stimulus. The other methods are bound to assume as a working basis that the ear responds in a certain way, e.g. that it behaves as an r.m.s. meter or that there is a constant relation between aural balance and deafening effect, but the aural balance method is the final court of appeal. As Tables 10 and 11, together with numerous subsequent tests, show that there is little difficulty in the use of the balance method, even by inexperienced people, we have recommended it as of general application. In this connection it is interesting to note that there are definite indications that the aural balance method carried out by alternate listening with a pure tone in free space (method D of Table 8) will be an international standard. We quite agree that there is a great need for an objective noise meter in order to remove the personal element as much as possible in cases of dispute, but it is only so far as the indications of such a meter agree with aural balance measurements that it is generally applicable. There is also the point that the cost of a reliable objective noise meter is many times that of an aural balance meter and usually precludes its use by any but the largest concerns. The most important point, however, which is dealt with in Section 4(*a*), is its failure to differentiate between a noise with a series of harmonic components and one without. The method is therefore quite unsuitable for the study of harmonic noises, e.g. transformer noise, and must be used with caution in other cases.

The objection that the aural balance method would be liable to error due to "tone deafness" on the part of the observer is not serious, as we have yet to meet a person who is sufficiently deaf to affect his readings without such deafness being known to himself and his associates. One should no more ask a deaf person to use an aural noise meter than a blind person to use a micrometer.

The fact that there is no fixed relation between aural balance readings and deafening, or masking effect, does not appear to deter a number of speakers from using the masking method to assess noise. They appear to consider it more important that some figures should be obtained easily by inexperienced persons than that the figures should have any general value for comparison with those obtained on other types of noise. This may be so in certain restricted cases, such as the routine checking of a large number of machines of the same type where the corresponding aural balance reading has been determined and will be used in relating the noise to any other. To create the position that two noises should be rated as equal because they have the same deafening effect on a particular reference sound, and even when they are clearly not equal in loudness when compared directly, is highly undesirable. No such anomalies arise in the use of the aural balance method as it is applicable to any steady sound, and, as it has been shown to be so simple that people using it for the first time get good agreement with experienced people, its superiority over the masking method for general use appears to be established.

The opinions expressed on the question of a distinction between loudness and nuisance show quite clearly that "one man's meat is another man's poison." Some, speaking of a transformer hum, complain of its monotonous continuity, while others are disturbed by the "irregular intermittence" of a lift motor. In the same way some dislike high-pitched noises and some low. This is clearly a point in favour of a subjective, as opposed to an objective, method of measurement, as it permits the investigation of such personal differences. We have tried on many occasions to obtain definite information which would confirm the reality of such preferences for certain types of noise, but when investigated quantitatively the results are inconclusive. There is no doubt a large field for psychological research on the matter and also into the general effect of noise on the nation's health and productive capacity. An obvious step, which is being taken in some quarters, is to carry out noise measurements in all cases of complaint in order to collect data for subsequent examination.

The question of where and how a noise measurement should be made on a piece of apparatus is mentioned by several speakers, apparently with a view to defining test conditions which would be generally applicable. So many variables are involved, however, that it is better to be guided by the use to which the results are to be put, bearing in mind that it is not possible, in general, to deduce with certainty the effect of changed conditions on the equivalent intensity of a noise. For purposes of comparison of the noise of one machine with a standard, or for analysing the noise into its constituents, there is much to be said for a test under "free space" conditions in a suitable laboratory. For predetermining the noise under definite conditions the safest procedure is to test under those conditions at the point where the noise is required to be known, or, if this is not practicable, to imitate the conditions as closely as possible, e.g. test an indoor transformer in a reflecting room and an outdoor transformer either out of doors or in an absorbent room.

The paper is criticized as dealing with only one aspect of the problem of noise measurement. A glance at the Table of Contents is a sufficient answer to this. It is true that Section (3), dealing with noise analysis, was made as brief as possible, but this was because it had formed the subject of a previous paper.* It was therefore sufficient to mention the improvements achieved since the writing of that paper, the general principle being unchanged.

Probably the point which has aroused more discussion than any other is whether or not the decibel scale of relative intensities is sufficient as a scale both of stimulus and of sensation, or whether it needs the help of a linear scale of sensation to create the right impression in people who are unfamiliar with the decibel scale and do not wish to study it. It appears from an examination of the discussion that, generally speaking, it is the engineers and those in close contact with them who are in favour of the loudness scale; while acoustic technicians are quite satisfied with the decibel scale, since they are used to it and think another scale would cause confusion. In any case, the lax use of the word "decibel" has made a change in nomenclature long overdue, so that an opportunity of clarifying the whole position is presented. We are in agreement with Dr. Davis's suggestion to use the

* *Journal I.E.E.*, 1930, vol. 68, p. 97.

decibrig as a logarithmic unit of general application, with the decibel as a special case when applied to acoustic intensities, and the phon for decibels above threshold at 800 or 1 000 cycles per sec. A noise will then be stated as "so many phons" when it sounds as loud as a pure tone of 800 or 1 000 cycles per sec. of the same number of decibels above threshold. A further scale of loudness need cause no confusion and, as many engineers have testified, would greatly assist by giving helpful instead of misleading impressions of relative loudness. Such a scale has in fact been proposed in America since the paper was written and is in fair agreement with Fig. 7, even though obtained in quite a different way. Also if the results on loudness estimates recently obtained by American investigators are used to construct a curve similar to that of Fig. 7 good agreement is again obtained. It is highly desirable, therefore, that all these results be examined and a representative loudness curve evolved. This is being done and the curve will be published in due course.

While the many cases of trouble from vibration or noise which have been described are very interesting, it is not possible to comment on them individually. The remainder of this reply will therefore be directed to points of general interest that have been raised.

Dr. Davis rightly says that "In almost all cases the object of noise measurement has been noise suppression." It follows that since we are interested in the mitigation of noise sensations we are interested in the measurement of the physical causes of these sensations only so far as the relation between the two is known. Dr. Davis is correct in saying that for a pure tone the number of decibels gives a "scale" of loudness just to the same extent that he may say that the angular deflection of the pointer of a square-law meter is a "scale" of current. The number of decibels is, however, no more proportional to the sensation of loudness than is the angular deflection of the meter to the current, and to insist on using the number of decibels as a measure of loudness is equivalent to refusing to calibrate the meter and persisting in quoting any current as "that producing an angular deflection of x degrees." Dr. Davis also says that "Nature works more or less on a logarithmic basis in the transmission, attenuation, dissipation, insulation, and absorption of sound." It is true that in passing through a partition the physical intensity of a pure tone (and of each pure-tone component of a complex sound) is reduced by a fixed number of decibels regardless of its initial intensity. If, however, we measure, in the case of a complex noise or a pure tone outside the 700–4 000-cycle range, the intensity of the equivalent pure tone on each side of the partition, we find that the change of the equivalent tone, i.e. the attenuation in phons, does depend upon the initial intensity as well as on the nature of the complex sound. Dr. Davis suggests measuring loudness by the number of phons. Nature certainly does not arrange that in "transmission, attenuation, dissipation, insulation, and absorption of sound" the change of phons shall follow the simple laws governing the change of decibels of a pure tone. Dr. Davis suggests that the effect of multiple sources is more "complicated" on the subjective scale. When this point is fully considered it is evident that the complication lies in the determination of the change in decibels and phons, particularly with complex sounds; the subsequent conversion of the phons into a subjective loudness figure is a perfectly simple matter. The fact that the subjective loudness scale does not assess two sources as twice as loud as one, or as having half the loudness at twice the distance, is natural. We have not heard it suggested that such relations hold. When information is asked on the relative loudness of two sources compared with one, it is useless to quote the physical change in decibels or phons, since the significance of a given decibel change varies enormously according to the initial level. Hence the decibel or phon scale is incapable of direct information regarding relative loudness. With regard to the assertion that the decibel scale is easily interpreted by a layman, we would refer Dr. Davis to the remarks of Messrs. Hoseason, Hutchinson, Ellis, Robertson, and Giles, and to the remarks of Dr. Kaye quoted by Dr. Hughes. With regard to the reasons for using Fig. 7 in preference to Fig. 4, a point which is also raised by Mr. Hoseason, the most important objection to Fig. 4 as a scale of loudness is that it assumes that the minimum detectable increments in loudness are all equal in loudness. The fact that Fig. 4 does not even approximate to Fig. 7 indicates that this assumption is not true. The point is being dealt with at length elsewhere.

In reply to Mr. Howe, the filament control in the oscillator of the portable noise meter operates only over a small range of filament voltage, and is quite permissible with small oscillator valves of the type used. The wave-form has been examined, and no harmonics of measurable magnitude are present. Replying to Messrs. Howe and Jones, we would point out that, as stated in the paper, all measurements of acoustic pressures were made under non-reverberant conditions with the microphone facing the source, and therefore the question of the polar diagram of the microphone does not arise. With regard to the effect of a reflecting enclosure, the figure of 5 decibels increase of level was not intended to be a maximum figure. The effect is clearly dependent on the volume of the enclosure relative to the size of the source and the absorption present.

Mr. Ayres desires an "agreed threshold," but it is surely clear that to be of utility this must at least approximate to that of the average person. The disadvantages of an "agreed threshold" at 1 dyne per cm^2 or at 1 lb. per sq. in. will be obvious. It was therefore necessary to determine the threshold intensities of the average person. With regard to the subjective scale of loudness, it is clear that extension in one part of the scale involves contraction in another. The vital point about the scale is that the loudness-scale number is proportional to the magnitude of the sensation. We would also point out that the thermometer carries a scale of temperature but not one of "warmth sensation," and it is common knowledge that the sensation of warmth experienced in a room does not depend merely upon its temperature.

Mr. Norris says that it is difficult to measure the noise from a large transformer. It has already been pointed out that it is only possible to measure the noise levels at chosen points, and in the case of transformers, as with

other apparatus, it is necessary to choose these points and to arrange the surroundings so that the measurements made will be relevant to the purpose in view.

In reply to Mr. Bull, we have applied the evaluation of Fletcher and Munson to various measured noises, but although it gives results which are usually closer to the measured values than those found by the rough method of "summed equivalent energy," large discrepancies still remain. Although this method of calculation is more complex than any of its predecessors, in our view it fails because it is still much too simple to simulate adequately the action of the ear. Mr. Bull suggests that the summed equivalent-energy figure shows greater disagreement with the measured value with a large number of components, and that the divergence between the two depends upon the number of components rather than on the harmonic relationship. We would refer to Table 3, where (1) shows a sound having 20 components and a divergence of only 1 decibel between the measured level and the summed equivalent-energy figure, whilst (5) and (6) have only 8 and 9 components respectively and show discrepancies of 31 and 28 decibels.

Mr. Hutchinson prefers the equal-loudness contours of Fig. 2 drawn as intensity levels instead of as decibels above threshold, and for some purposes this will be the more convenient form.

Dr. Hughes asserts that "experience in America did not suggest the necessity for a subjective scale" of loudness. We would refer him to the references given in the paper. With regard to his statement that we have not referred specifically to objective methods of noise measurement, we would refer him to the paper itself. We are in complete agreement with the remarks of Dr. Kaye which Dr. Hughes quotes. It is just because "the decibel scale of noise levels is one to be learnt by personal experience" that a subjective scale is necessary. Dr. Hughes concludes by saying that "To be a fact and not an impression, noise must be measured objectively." Since noise is a sensation it is necessarily both a "fact" and an "impression," and a sensation obviously can never be measured objectively. This does not preclude the measurement of some function of the stimulus which may be proportional to the sensation, but the mere determination of such a function, quite apart from the construction of an instrument operating to the same law, requires a knowledge of the response of the ear to complex tones which does not yet exist.

In reply to Mr. Devey, the observer using the noise meter has one ear open and presented towards the source. We would refer him to the reply to Mr. Norris.

Speaking of the threshold determinations Mr. Williams asserts that the authors agree that the methods which he classifies as (*a*) give two different results. We do not agree. These methods give four different results. In selecting which value of threshold is to be measured it is obvious that the most relevant criterion of threshold must be chosen, and the reasons for selecting our method are very clearly set out at the beginning of Section (2). It must in any case be realized that divergencies between the four threshold values are not large enough to be of much practical importance. Mr. Williams also complains that "the authors give very little information . . . on the method of averaging." Since the methods of determining modes and deviations may be found in textbooks it was not considered necessary to include them in the paper. It is obvious that considerations of space preclude us from giving all the mode curves, as Mr. Williams desires. The degree of "normality" of a distribution can be inferred from the values of the mean and standard deviations, and it was for this specific reason that both values were given.

Prof. Scholes inquires about the use of the noise analyser. This apparatus can readily be used *in situ* and has been in constant use for the examination of the noise emitted by various types of machines both on site and in the laboratory. A complete analysis occupies from 10 to 30 minutes, according to the number of components present in the sound. The analysis of the sound from a machine enables us not only to find those components which are loudest and which must therefore be dealt with first but also to determine which part of the machine produces them.

In reply to Mr. Taplin, the lining of the test rooms is provided to eliminate sound reflection, and the motor shown on test in the slide was bolted to a bed-plate mounted on a large concrete block not connected to the floor and having foundations separate from the rest of the building.

In reply to Dr. Prescott, the whole question of the relation between loudness and annoyance is at present very little understood and requires further investigation. This in itself is a strong reason for deprecating the use of "objective" meters. In using the aural balance method it is always open to the observer to determine separately the intensities of the standard tone which are respectively as loud and as annoying as the noise under investigation, provided he feels that he can make the distinction. Before the distinction can be examined it will clearly be necessary to define what is implied by the words "loudness" and "annoyance."

Mr. Chadwick suggests that the thickness of the air space between two walls is important since the vibrations on the one wall will be transmitted to the other by the compression and consequent pressure-rise of the air film. This would be correct if the air space were indeed a film; that is, if its thickness were comparable with the amplitude of vibration of the inner wall. It would not be practicable, even if it were desirable, to build brick walls with a spacing of much less than 2 inches, and this is so large compared with the vibrational amplitudes involved that the two walls behave independently.

The methods used in calibrating the condenser microphones are as Dr. Say suggests, and very satisfactory agreement has been found between the various methods in the range of overlap.

In reply to Mr. Durand, the question of the transmission of noise into the test room along the shafting has of course been examined, and the necessary steps have been taken to avoid error due to this.

In reply to Mr. Woodward, the frequency range of 100–6 400 cycles per sec. was adopted since this range covers almost all the noises encountered in the industrial field. We would point out to Mr. Ross that we do not "stress the liability to error of observation when dealing with harmonic sounds." The point stressed was that in these cases the value obtained by the method of summed

equivalent energy and by the ordinary "objective" meter shows a large error when compared with the observed value. The "liability to error" lies not in the aural balance measurement but in the use of the energy-summation principle.

In reply to Mr. Stokes, the fundamental frequency of the noise from a 50-cycle unpolarized transformer is 100 cycles per sec. since the forces between the laminations of the core depend upon the magnitude of the flux and not upon its direction. The non-linearity of the ear introduces harmonics and sum and difference tones, but not sub-harmonics.

In reply to Mr. Fawssett, Fig. 1 shows the threshold pressures, that is (in this case) the largest pressures which are inaudible. The curves showing the pressures which produce tones of the same loudness at different frequencies may be found by compounding Fig. 1 and Fig. 2.

The question of bone conduction is raised by Messrs. Brookes and Morris. There are two aspects involved; the transmission of energy by bone conduction from one ear to the other, and the transmission of energy from the telephone case direct to the inner ear. Regarding the first, the attenuation introduced by the bone conducting path is about 50 decibels. At the position of balance in the aural balance method the intensities in each ear are comparable, so that the introduction of a tone attenuated by 50 decibels is of no importance, and, in general, it will be completely masked. With regard to the second point, this possibility is reduced by the use of a soft rubber sealing cap on the telephone; direct measurement shows that the energy reaching the ear by means other than the pressure generated in the ear canal is at least 60 decibels below the latter, and so does not affect the level as determined by measurement of the pressure in the ear canal.

In reply to Mr. Bray, we have not been "trying to arrive at a straight-line relation between stimulus and sensation." Only the uncompromising advocates of the decibel loudness scale pursue this course. We have been trying to find what that relation really is: hence the subjective scale of Fig. 7. With regard to the use of this scale with multiple sources, we refer Mr. Bray to our reply to Dr. Davis.

In reply to Prof. Parker Smith and to Mr. Macdonald, the use of oscillograms to study sustained noises was investigated and discarded by two of the present authors as long ago as 1926.

TENTH FARADAY LECTURE.

"THE ENGINEER AND THE FREE ELECTRON."

By CLIFFORD C. PATERSON, O.B.E., Past-President.

[*Lecture delivered before* THE INSTITUTION 15*th March, before the* SOUTH MIDLAND CENTRE 31*st January, before the* MERSEY AND NORTH WALES (LIVERPOOL) CENTRE 12*th February, before the* NORTH-EASTERN CENTRE 14*th February, before the* NORTH MIDLAND CENTRE 15*th February, before the* NORTH-WESTERN CENTRE 27*th February, before the* WESTERN CENTRE 13*th March, before the* IRISH CENTRE 13*th April, and before the* SCOTTISH CENTRE 11*th May*, 1934.]

The science of electrical engineering was born again when the physicist showed how electricity could be liberated from metal. Except in lightning—that uncontrolled and terrific manifestation of electricity—few people used to think of it other than confined in metal or in some other material. It is true that in this imprisoned state electricity is still the most valuable servant which mankind possesses. Confined to its metal conductors it still lights our houses, drives our vacuum cleaners, heats our electric fires and cookers, runs our electric trains, and performs a hundred other services. When liberated, however, electricity shows potentialities of which no one had dreamed in that more pedestrian period which preceded the present century, and which indeed ran on till the War.

It is generally, or anyway it used to be, several years before a scientific discovery found practical application in everyday life, and this new free state of electricity was a mystery of the physicist for nearly 10 years before others took serious notice of it.

It all started with the discovery by Sir Joseph Thomson in 1897 of the electron, that minute ultimate constituent, which is to electricity in the mass what the grain of sand is to the whole sea-shore. After Thomson, electricity had no longer to be studied and thought of in the mass. It was made up of myriads of separate electrons whose characteristics could be studied. Just as physiologists had learned that disease could be envisaged in terms of isolated germs and their life history, so the physicist had found that electricity could be thought of in terms of the individual electron, its habits and affinities. The discovery by this great Cambridge physicist was hardly noticed by engineers until well into the present century. Shortly before the War, however, there were a few who were struck by the new theories and experiments. One of the first of these people was Sir Ambrose Fleming. He had some knotty problems to unravel connected with the behaviour of ordinary electric lamps, which used to "flash" and destroy themselves for no apparent reason. I remember well his talks on the subject. The new theories helped him, and furthermore led him, as an engineer, to think how this new knowledge of the physicists might be used in other ways. So he became one of the pioneers of free-electron engineering, which has opened out for electricity a new era of conquest and service. The developments from this, which I want to discuss this evening, are revealing possibilities in the future, to which few would be bold enough to set a limit.

If we look broadly at the applications of electricity we shall find that the reasons for its universal adoption have been, first its transportability, and secondly its ease of control. Extensions of its field of service will usually be found to have followed some elaboration of one or other of these two qualities.

I am not dwelling this evening on the aspect of transportability. I would only recall to you such national services as the telephone system, by which electricity, carrying our speech, is directed to any corner of the land: or our power system, by which electricity generated 100 miles away is used for boiling the kettle for tea or for driving factory machinery. Nor should one overlook the electricity conveyed to our broadcasting aerials, which can awaken instantaneous responses at the other end of the world. This country has taken a pioneer place amongst the nations in establishing the electrical telephone, power, and broadcasting services, on sound principles and on a national scale. This mere mention will, I am sure, suffice to impress us with the aspect of the transportability of electricity.

It is rather in the direction of control of electricity—of making it do what one wishes—that the free electron has given the engineer such new and extraordinary powers.

Electricity as Faraday knew it and as engineers knew it up to about the War period, was electricity which obeyed simple laws, or did so very nearly. Electricity flowed nearly always in metal wires—and we controlled it by changing the resistance of such circuits or by changing the voltage across them. The engineer did not know precisely what the electricity was, but he found the current very easy to control in this way. It could be switched on or off with a simple motion of the hand, or it could be made to decrease or increase proportionally to the resistance and the voltage of the circuit.

Great was the thrill to the engineer when he realized that the familiar amperes—the electric fluid as it was once called, the "juice" as it is known by the irreverent to-day—that these amperes actually consisted of minute elements of electricity (electrons) of which 6 290 000 000 000 000 000 per second go to 1 ampere; or, in other words, make up sufficient electricity for lighting one good street lamp for 1 second. To him,

however, this was but an academic fact. It had no special importance because these electrons were all confined to metal wires or occasionally to liquid conductors. The electrons were not free. No effective way had been put into his hands of making these electrons leave the peaceful paths of metallic conductors.

It was only when it was shown to him what wonderful effects could be produced when this stream of electrons was made to leave the conductors that the engineer set to work to develop and to use every kind of free-electron agency and device. This is the tendency in electrical engineering which, since it started about 20 years ago, has revolutionized many things in life, and, I am convinced, has many more surprises in store.

The secret of the revolution is that a stream of free electrons, whether in a vacuum or a gas, can be manipulated with such facility that the electricity can be increased or decreased at a rate of millions of times per second or alternatively as slowly as desired. It can be reversed or stopped equally quickly. It can be modulated automatically in the most complicated ways, and no limit is set to the amount of energy which can be so controlled. Finally, whilst the agency which imposes this control on the electron stream is usually itself electrical, it is possible to make light from an ordinary lamp, or magnetism, or heat, the controlling agencies.

It may be asked, why are these extremely rapid actions of the electrons wanted? What is the practical use of them? It is my object now to explain, and in a few minutes to demonstrate, if I can, the wonder of them. Suffice it to say now that much of what we see and hear consists, if analysed, of extremely rapid happenings. The eye and the ear are quite unconscious of these high-speed fluctuations and vibrations, although sensitive to them. They transmit to the brain only the mass effect of them. Just as the pistons of a motor-car engine are sensitive to the thousands of explosions per minute which make the engine rotate; but you, sitting in the car, are only conscious of the running of the engine and the fact that the car is moving under your control. You do not appreciate the complicated train of events which goes to make up each individual explosion in a cylinder of the engine.

So, all unknown to us, the machinery of our eyes and ears is responding to, and following in detail, rapid and complicated oscillations of which we have no conception apart from the investigations of science.

I shall have to ask you to follow me whilst I describe two of these electron-liberator devices. I want you to observe that the door which opens the passage and lets the stream of electrons out of the metal circuit is nearly always an incandescent part of it, heated to such a high temperature that electrons emerge freely. That is what the filament, or in other words the cathode, in a radio valve does.

I will not burden you with theories of the mechanism of this process. I will merely remind you that when electricity passes through a hot filament, or other white-hot substance, it is like water which has hitherto been passing through a watertight lead pipe, coming to a length of worn-out porous hose. The water will soak through it, and if there is a good pressure will squirt out in a number of minute jets. As you know, electric circuits usually consist of metal wire. So long as these are cold the electrons are confined rigidly within the wire as they travel along it. If, however, we include in the circuit a piece of red-hot wire the electrons will be released from the interior of the wire and swarm around in a thin layer on the outside surface, ready to be taken elsewhere the moment they are attracted away by externally applied electrical forces exerted from another metal electrode nearby. Just in the same way as the molecules of steam from hot water, after experiencing some difficulty in getting clear of the water surface (as the existence of the latent heat shows), swarm in the space above as a saturated vapour unless they are allowed to pass away.

It is at the instant during which the electrons are travelling from the hot solid through the short space—usually less than 2 inches—to the other electrode that the control is exerted which causes them to comply with the most exacting demands, flowing and ebbing, accelerating or slowing up with unthinkable speed and precision. They are controlled by the electrical dictates of this electrode as a complicated orchestra is controlled by its conductor.

The device in which this operation is carried out most perfectly is the well-known radio valve. It is so perfect because the speed at which the electrons can be made to manœuvre is so high. They can be made to change, to reverse, or to oscillate, so rapidly that the complete stream of electrons will flow back and forth if necessary 300 000 000 times per second.

Now I should be grateful if you would bear in mind this radio valve and its power of manipulating the free-electron stream, whilst we turn for a moment to examine a second device for liberating electrons and rendering them free of the materials in which they normally congregate. This is the photo-electric cell. The cell looks much like an ordinary wireless valve, but it is entirely different in its function. For the electrons are liberated not from a hot filament, as in the valve we just considered, but from a specially sensitized cold surface which has the property of releasing electrons only when light falls on it. The cathode is the sensitive surface, and the anode is the collector of the electrons which leave the cathode so long as light is falling on it. If you put the cell in the dark no electrons leave the cathode for the anode. The cell is dead. Let in ever so little light, however, and the electrons instantaneously begin to leave the sensitive cathode. The number of electrons so set free is very closely proportional to the intensity of the light. If we double the light we double the number of free electrons.

The number of electrons which flow is extremely small, but that does not matter when we have at our command amplifying valves of the kind we first considered, which will magnify their number and their exact fluctuations to almost any extent, and with the highest precision.

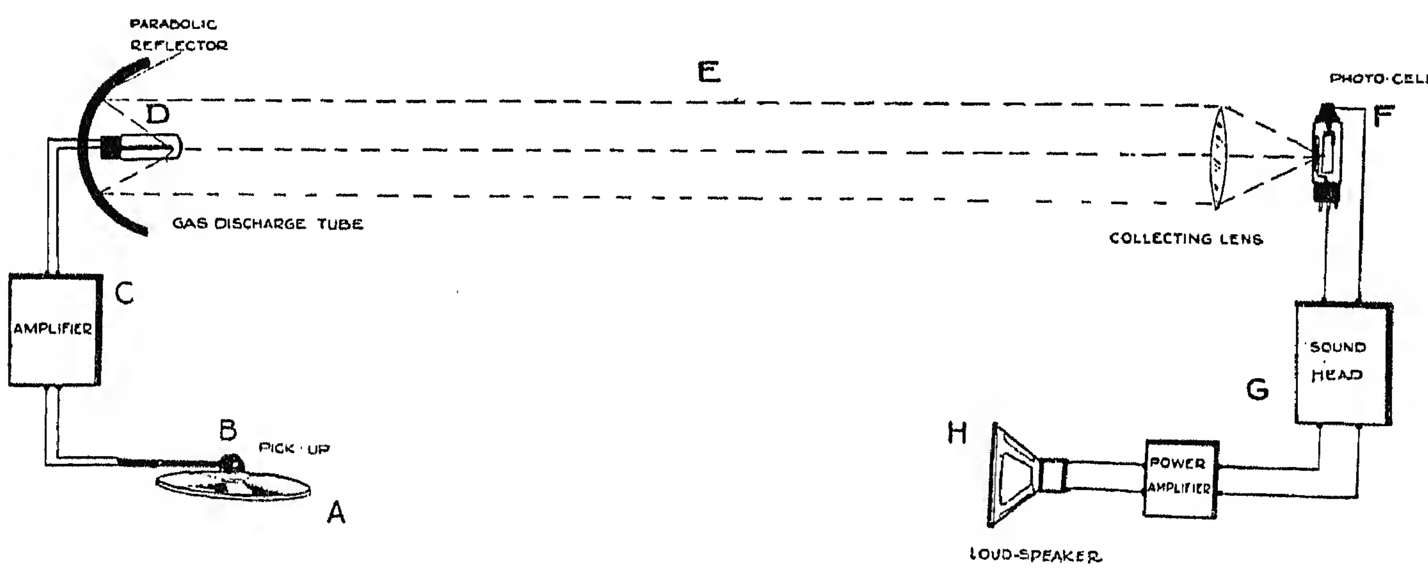
These photo-cells are used to the fullest extent of their possibilities in the reproduction of talking films. They are also the key to television. In some of the latest television devices the photo-cell has to receive over 300 000 impulses in every second. That is to say, light is turned on to the cell only for 1/300 000th of a second, yet the cell must respond accurately to it and

emit electrons and be ready immediately after to receive a new light impulse and emit a new flight of electrons; and so on 300 000 times per second.

I could enumerate many instances where the photo-cell saves time and money, but I have indicated enough to show the future of this free-electron device which enables nearly everything to be done automatically which man now does by virtue of what he sees.

Now I have been explaining these two free-electron devices—the radio valve and the photo-cell—because they can be made to illustrate the wonderful way in which free electrons can be used and controlled in modern electrical engineering. The illustration I am taking is in connection with the amplification, conversion, and reproduction, of sound.

I said just now that what we hear consists of very rapid happenings. The sound of my voice reaching your ear, of what does it consist? It is composed of very complicated pulses carried by the air between you and me. What is such a pulse? It is a travelling disturbance of the pressure of the air made by the breath



as it passes over the vocal organs and issues from one's mouth. The character of the sound you hear depends upon the minute variations which take place during the pulse. We can show these pressure changes in the same sort of way as the barometer chart shows the much slower variations of atmospheric pressure. (The lecturer here showed two graphs, the first representing the simple vowel sound " A," as in " father," and explained that the sound one hears consists of the repetition of this complicated pulse 100 times per second. The second one showed the word " poor," occupying about $\frac{1}{5}$ second. Graphs were also shown, illustrating the wave trains given respectively by the clarinet and the oboe; the detailed forms of which give them their distinctive sound character.) If we want to reproduce, transmit to different places, or magnify, such a train of air pulses we must do so by manipulating electrons so that their electrical fluctuations accurately reproduce these high-speed effects.

You see, we must make exact electrical copies of these sound pulses. When the pulse is powerful, more electrons pass; when it quickly diminishes, the number of electrons diminish in proportion. The art of doing this is the basis of the long-distance telephone, the talking picture, broadcasting transmission and reception, trans-oceanic telephony, and a hundred other uses less important perhaps at present, but growing in importance as our technique develops.

Whilst we had the telephone and microphone which permitted us to make exact electrical copies of sound waves which could be sent long distances over wires, we had, except for the gramophone record, no means of recording and reproducing speech and music till the free-electron device in the form of the radio valve and the photo-cell supplied the means.

Look at the train of operations shown in the Figure, operations which I want in a moment to show you working. We start with a gramophone record A, which must take the place this evening of an orchestra or of a person talking. This has recorded on it—in mechanical impressions on wax—the elaborate pulses such as we have just seen of music or speech. With its needle and electrical pick-up B we convert these impressions into exactly equivalent electrical impulses. These impulses are faithful copies, but very weak, so they must be amplified in the apparatus C. This contains radio valves, in each of which in turn and with inconceivable speed the free electrons are made to copy exactly in their own movements the impulses impressed on them. Each successive radio valve multiplies the number of electrons actuated by the previous one by such a factor that, for each electron in the accurately manœuvring swarm entering the amplifier, there will be 10 million electrons in the equally accurately manœuvring swarm emerging from it. The impulses of this swarm are then impressed on the lamp D, which converts them from electrical impulses into exactly equivalent modulations of the beam of light E travelling to the right-hand side of the Figure. Here the beam of light, still bearing within it these modulations, is collected by the lens and focused on to the photo-electric cell F. We saw that when light falls on one of these cells, electrons are set free within it exactly proportional in number to the intensity of the light. So the photo-electric cell F responds accurately to the modulations, and converts them again into electric impulses. As they are very weak, however, they have to be passed again through an electric amplifier G with its radio valves, ready to be converted finally into sound

waves in the air by the loud-speaker H. This will reproduce the sounds you are to hear, and which originated with the gramophone record A.

This, of course, is very similar to the chain of events in every talking film, except that the film with its sound record is interposed in a steady beam of light in place of E. In the whole train here, including the amplifiers, the impulses have been converted and re-converted six times, in addition to being passed through nine stages of amplification. Perhaps the most dramatic interest lies in the link from D to F, the transition from electric to light impulses and back to electric.

(The lecturer then demonstrated the actual apparatus using a beam of light about 25 ft. long between projector and photo-cell. He first rapidly varied the light entering the cell with a hand torch, which resulted in an unpleasant noise from the loud-speaker. He then controlled the light variations in a less crude manner by superposing on the current in the lamp the musical currents from the pick-up of a gramophone record playing Purcell's "Trumpet Voluntary." This experiment exactly reproduced the train of apparatus described. The audience heard the music from the loud-speaker as long as the light was focused on the cell. When the light beam was interrupted by the hand the music stopped, or was modulated in volume at will.)

Is it not very wonderful that we are able to translate at will such an enormously rapid and complicated train of waves which make up human speech or the music of instruments, into electric impulses or light variations, or photographic or mechanical impressions, and lose nothing as regards faithful reproduction after all these metamorphoses?

After this it may not be so difficult to realize that free-electron apparatus is now being devised in which the inertia of the electrons actually plays an essential part in the creating of oscillations which can have a frequency of over 3 000 000 000 per second. Such oscillations are so rapid that we can begin to treat them as if they were radiations from a source of light. For you must remember that they are of the same nature as light. We can put a small aerial a few inches long in the focus of a reflector to radiate them—much as we put a lamp in the reflector of a motor-car headlight to concentrate and radiate light—and our wireless beam is projected forward like that of a headlight. The possible use of wireless beams of this frequency is to send speech or signals over short distances with simple and compact apparatus.

We can imagine best what 3 000 000 000 oscillations per second means by picturing a train of waves of two ups and downs each inch. Three thousand million would just about reach round the world at the Equator. That is the number of oscillations of the electrons which we can reach, if we want to, in our free-electron valve every second.

What uses will be found for these very short-wave oscillations is a matter of the future, but probably of the very near future. Used for radio purposes they provide a new and inexpensive means of communication with a relatively inaccessible point at a distance of some few miles. It may be across a strait, or on an island, or amongst mountains. For this immediately practical purpose they have been tried already with success.

In a more general way, however, the existence of such a new tool soon leads to the discovery of uses for it by interested investigators. An unexpected use of these oscillations, of which more may be heard, is in the electromedical field, where a means of producing local warmth in the inner parts of the body has long been needed for therapeutic purposes.

May I now interpose a brief parenthesis? It would be wrong for me to leave with you the impression that we understand all about the electron. To-day we know we do not understand it so well as we thought we did three or four years ago. Nevertheless, we know its speeds of travel; we know its weight as a travelling missile; we know how many electrons there are in any given quantity of electricity, and the exact number which goes to form the elemental structure of every substance we know of; and we know that it is only one hundred millionth of the size of the smallest particle whose shape the most powerful microscope will reveal. There is, however, one very disturbing enigma about the electron: we have difficulty in satisfying ourselves as to its form and substance.

We have been speaking of it as if it were a minute particle, which might look like a little sphere of glass if it could be magnified sufficiently. It is true that it sometimes or often behaves as if it were as solid and definite a particle as this. Under other conditions, however, its behaviour is such as to make it necessary to picture it as if each electron were a minute packet or group of undulations or waves, and the puzzle is that it appears to be entirely indifferent whether it has the particle or the wave characteristics. At first sight, therefore, it looks as if we can say that we know what the electron is, but in another sense it seems that we don't know how to comprehend it.

As, however, thinking humanity comes to realize more and more that things are not always, or even usually, what they seem, apparent contradictions such as this should no longer trouble us. So it is with the electron. Whenever the electron acts on, or influences, anything else at a distance it has to be treated as a wave. All action at a distance is by waves. Electrons, like light, act at a distance, and therefore they have to be regarded as waves. Whenever anything arrives anywhere, however, it behaves as a particle. So when the electron, travelling in its wave character, arrives anywhere or collides with something and its position is momentarily located, it automatically assumes its particle character and behaves like a definite and solid object. It is a help when we realize that electrons are not alone in this. For instance, light, which we regard as travelling waves, behaves like particles when arriving at a surface.

Another, perhaps more metaphysical, way of saying the same thing is to introduce the modern idea of probability. So long as any travelling thing has not arrived it has only a probability of arriving. All probabilities are determined by wave functions—that is a fundamental mystery. Once it gets there, however, there is no longer any probability about its being there; its presence is a fact, and facts behave like particles.

So you see there is no real inconsistency about the dual aspect of our electron. You decide which aspect

to use by the formulation of your question. Broadly, if we ask what may happen, we must think in terms of waves. If we ask what has happened, we must look upon the electron as a particle.

"What?" you say. "If an electron is a particle only before it starts and after it arrives, and a wave whilst in flight, why does not the same principle apply to a golf ball?" How do you know it doesn't? "Well," you say, "if it hits me on the head I can feel it is not a wave!" Ah, no! It has then arrived somewhere and it is a particle.

The new theories of wave mechanics show that the uncertainty of state and position increase with the smallness of the entity and the speed of its travel. An entity of the extreme smallness and great speed of the electron is a limiting case, and the two states—particle and wave—are extremely well marked. The golf ball, millions of millions of times larger and infinitely slower in motion, has very little difference left between its stationary and moving condition. Nevertheless, we cannot admit that even the golf ball is exactly the same in flight as at rest. The mathematical physicist requires that there must still be an element of uncertainty as to its whereabouts and its character when it is in flight.

I want to speak now of an entirely different branch of electrical engineering, which is utilizing the free electron in astounding ways in its latest achievements. Until a few years ago the electric lighting engineer had as his instrument of lighting little but the electric filament lamp. His procedure in getting light from this is to make the electrons travel along a fine wire inside the electric bulb and to crowd the electrons together so much that they make the wire white hot—so hot that it gives out light. Whenever electrons escape from the wire in this kind of lamp they tend to harm the lamp, and every effort is made in this case to keep them inside the filament. This is the lamp which we still use to-day and shall continue to use for a long while yet.

Recent advances in technique, however, have shown that brilliant lighting effects are to be obtained by releasing the electrons from the hot electrode, and allowing them to do what they like with the gases and vapours which we put in the bulb and across which they shoot with enormous velocities. All these effects are the result of emotional encounters between electrons and gas atoms under the specially prepared conditions of our glass bulbs and tubes. Encounters like these, which take place when the electrons are travelling at speeds up to 6 million miles per hour, must not surprise us if they yield bright effects.

I am not suggesting that this manifestation is new. It was observed by physicists over 200 years ago, and has sometimes been utilized. With the understanding which physics has given the engineer of the way that electrons can extract light from gases and vapours, however, lamps have been devised which yield several times more light for the electricity consumed than filament lamps can hope to give. So the free electron opens up a new vista also in the field of electric lighting.

In order to obtain these high efficiencies we have to learn firstly how to open the door to let electrons out in the most effective way, and secondly how to make them play in the most efficient manner on the gases in the bulb, because it is these which yield the light. I will now trace the more recent stages in this development of electric discharge lighting.

The post-war period has exhibited a wonderful growth of colour at night in the main streets of all our large cities. If the effects are not always beautiful they are at least cheerful. (The lecturer then showed four lighted tubes of varying colours and described how they are produced by applying high voltages to them—voltages too high to be safe without essential precautions.) No other way was thought of for coaxing the electrons out of the two metal electrodes in the tube but that of sucking them out, as it were, by the brute force of high voltages. Even then the stream of electrons between the electrodes is not copious, but it suffices for display signs and advertising designs, for which an unduly high brilliancy at night would be a disadvantage. They yield about 1 candle per square inch for a neon tube.

When these electrodes were made in the form of red-hot material, however, striking results followed. Hot material constitutes such a wide-open door for the electrons that the flow is copious and it takes place at low voltages. The brightness of the column of light is also greatly enhanced, rising to nearer 10 candles per square inch. (Three more tubes were shown of similar colours to the last but of greater brightness; and a slide to scale showed a cold and a hot cathode tube of equal candle-power, the hot tube being only about $\frac{1}{22}$nd of the length of the cold tube.) Units made with such lamps are of commercial value, although their use is probably limited to colour floodlighting and the like, where no alternative for obtaining coloured light is found to be so economical.

Some here may be asking whether colour floodlighting is only another device for making night hideous. That depends on the colours and on the objects illuminated. To illuminate grass with green light and red brick buildings with red light is to bring out their natural colours at night in a way which is not possible with white light.

Experiments with such tubes led to important further advances. It was realized that if conditions could be found for passing electrons through certain vapours, such, for instance, as sodium, enormous yields of light ought to be possible. This favourable result is obtained because so much of the energy of the electricity is sent out by sodium vapour in radiation which stimulates our vision instead of being emitted as heat, as is usually the case with artificial light sources. This shows how it is that vapours and gases can be more efficient light-givers than hot filaments. (A cover was here removed, showing a lighted sodium lamp, and the lecturer continued.)

The sodium vapour has to be very hot, however, and at these high temperatures it is corrosive. So research on special glasses has been necessary in order to find one which would resist the action of the vapour. This has now been achieved and lamps such as this are the result, which show initially an efficiency of 3 or 4 times that of our present filament lamps. The light from the sodium lamp is of such a pure yellow that at present we can think only of its use for the lighting of arterial speed roads, where æsthetic considerations can be ignored. All objects appear as a monotone in yellow-brown.

The study of the sodium lamp has led to further advances, and has indicated the possibility of still better things. If we only put sodium atoms in the path of the electrons for them to play with we can only hope to obtain light which is characteristic of sodium. There are other gases and vapours, however, each of which will emit its own characteristic hue when acted upon by the electron stream. Some are intrinsically efficient but have a bad colour. Others have a good colour but give little light for the electricity they consume. Some promising ones are left, and some mixtures, but only a few, are possible. Each gas or vapour requires its own conditions for use, and these have to be ascertained by research, and by developing materials appropriate to each.

In one of the latest discharge tubes, which uses mercury vapour as one of the gases for electrons to act upon, the filament transformers and the starting coil are eliminated altogether, and the lamp is reduced to very simple proportions. (The mercury high-pressure lamp was uncovered here.) This lamp gives well over twice the light of our existing filament lamps for the same electricity consumed. The watts are 400, so that for 400 watts we get as much light as with the present 1 000-watt lamps. The colour—although deficient in red and therefore not yet like enough to daylight—is sufficiently good for use in many outdoor situations. These lamps are beginning to be adopted in considerable numbers for lighting our streets.

I have shown you these lamps not because they represent finality: I am quite sure that they have not reached their final forms: but I wanted you to see how the physicist and the engineer working together are learning the ways of the free electron in this new sphere of making light, and are giving us new lamps wherewith to lighten our hours of darkness.

Michael Faraday, to the honour of whose memory this annual lecture is delivered, was one who loved his research because of the marvels it revealed; and he loved most of all to demonstrate these marvels to others. He was the founder of electrical engineering, and I like to think that we electrical engineers who follow him, and who benefit by his genius, may have caught and retained something of his vision, of his modesty in the presence of the marvels which we handle in our daily work. (A portrait of Michael Faraday was thrown on the screen as the final tribute was being paid to him.)

I feel I shall have succeeded in my lecture this evening if I have, even in a small measure, stimulated anew a sense of wonderment, if not of reverence, in the face of this sub-atomic world of complicated electrical activity: a world far below the power of the microscope to see, but which, as we understand it better, can be made to aid our civilization still further and give men and women of our own and future generations a fuller life.

How Faraday himself would have loved to have given the Faraday Lecture to-day! What a lecture it would have been!

COPPER-OXIDE RECTIFIERS IN AMMETERS AND VOLTMETERS.

By EDWARD HUGHES, D.Sc., Ph.D., Member.

(*Paper first received* 15*th January, and in final form* 23*rd December,* 1932; *read before the* METER AND INSTRUMENT SECTION 2*nd March,* 1934.)

SUMMARY.

This paper deals with various errors that may arise when a moving-coil instrument is used in conjunction with a copper-oxide rectifier to measure alternating currents and voltages.

The effects of circuit impedance upon the reading of a rectifier ammeter and of the amount of series resistance upon the reading of a rectifier voltmeter are investigated. It is also shown that the effective resistance of a rectifier as calculated from tests with different series resistances varies considerably, owing to the distortion of the current wave even when the voltage is sinusoidal.

The use of rectifier instruments having linear characteristics to measure very bad wave-forms, such as telephonic currents, is dealt with; and it is shown that such instruments, calibrated with a sine wave, read the effective value within about 10 per cent for any shape of wave, however much it may be distorted.

Finally, the difficulties encountered in the application of rectifier instruments to current transformers are examined and methods of reducing those difficulties are suggested.

(1) INTRODUCTION.

For the measurement of small alternating currents and voltages the copper-oxide rectifier used in conjunction with a moving-coil instrument possesses far greater sensitivity than any other arrangement. As this combination is likely to become even more extensively used in the future, it is desirable to investigate the errors that may arise and to suggest methods for their reduction. In view of the work already done by Hartshorn,* Sahagen,† and others, on the effects of frequency and of temperature variation, this paper is confined to an investigation of the following factors.

(1) Effect of impedance in series with the rectifier when the latter is carrying the whole current. (2) Equivalent resistance of the rectifier. (3) Effect of series resistance in rectifiers used with voltmeters. (4) Effect of wave-form with a rectifier instrument having a linear characteristic. (5) Rectifier instrument used in conjunction with a current transformer.

The majority of the tests described were carried out on 10-mA and 1-mA instrument-type rectifiers kindly loaned by the Westinghouse Brake and Saxby Signal Co. Several units of each type were tested, but in most cases the results for any one type agreed fairly closely with one another. A number of tests were also made on 5-mA rectifiers, but as the results were very similar to those obtained on the 10-mA units they have been omitted from this paper.

(2) EFFECT OF CIRCUIT IMPEDANCE.

A rectifier was connected in series with a vacuo-junction J and a non-inductive resistance R across a low resistance r, as shown in Fig. 1. A large air-core choke X was connected in series with r so as to ensure the current wave being as sinusoidal as possible. The resistance of the vacuo-junction was 12·3 ohms and that of the milliammeter was 5 ohms. The wave-form of the alternator voltage was almost sinusoidal, and the current through the rectifier was only a very

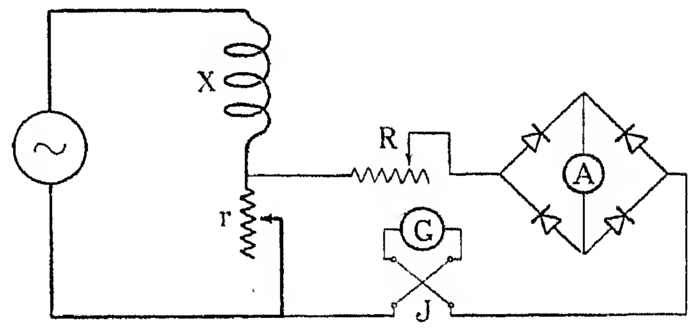


FIG. 1.

small fraction of that through r, so that there could be practically no distortion of the potential difference across the latter. For a given value of R, a range of readings on A and G were taken by varying r. If the rectifier and milliammeter A had been calibrated with a sine wave of current to read the effective value, the scale reading thus obtained would have been 1·11 times

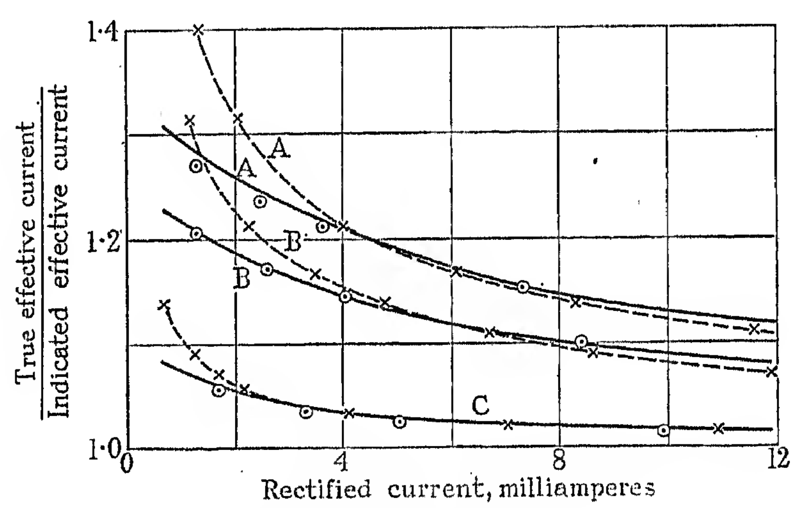


FIG. 2.—10-mA rectifiers.

— Experimental results.
⊙ Values calculated from d.c. resistance.
× Values calculated from expressions (2) and (3).
A. 17·3 ohms in series.
B. 57·3 ohms in series.
C. 617·3 ohms in series.

the actual rectified current. The combination J and G had been calibrated by being connected in series with the milliammeter A across a d.c. supply.

In comparing the behaviour of the rectifier instrument under different conditions, it was found best to plot as ordinate the ratio of the true effective current, as read on the thermal instrument, to the effective value indicated on the rectifier instrument when calibrated with

* *Proceedings of the Physical Society*, 1930, vol. 42, p. 521.
† *Proceedings of the Institute of Radio Engineers*, 1931, vol. 19, p. 233.

a sinusoidal current. This ratio may also be expressed as (True effective value of current)/($1 \cdot 11 \times$ mean value of current), or as the form factor of the current through the rectifier divided by $1 \cdot 11$.

The full-line curves in Fig. 2 show the mean of the results obtained on two 10-mA instrument rectifiers, the values for the two rectifiers being practically iden-

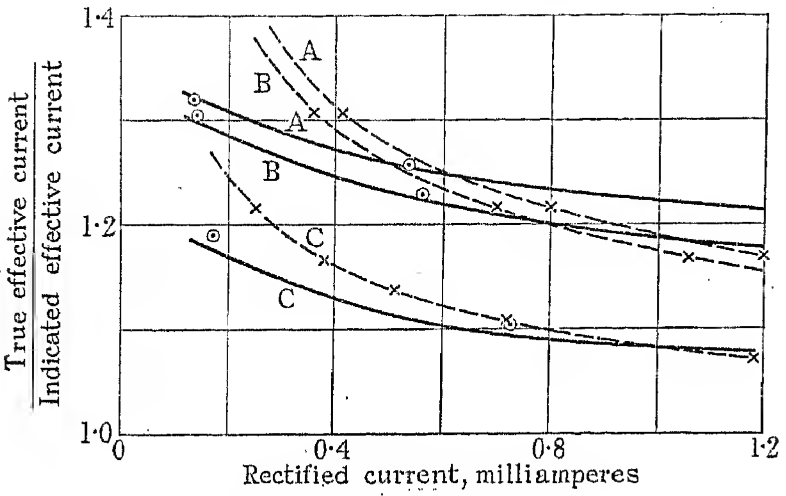


FIG. 3.—1-mA rectifiers.

— Experimental results.
⊙ Calculated from d.c. resistance.
× Calculated from expressions (2) and (3).
A. 17·3 ohms in series.
B. 57·3 ohms in series.
C. 617·3 ohms in series.

tical. Corresponding curves showing the mean results obtained on the 1-mA rectifiers are reproduced in Fig. 3. It will be seen that for a given reading on the rectifier milliammeter there is a considerable variation in the true effective value of the current for different values of the circuit resistance. This error is due to distortion of the current wave caused by the variation in the

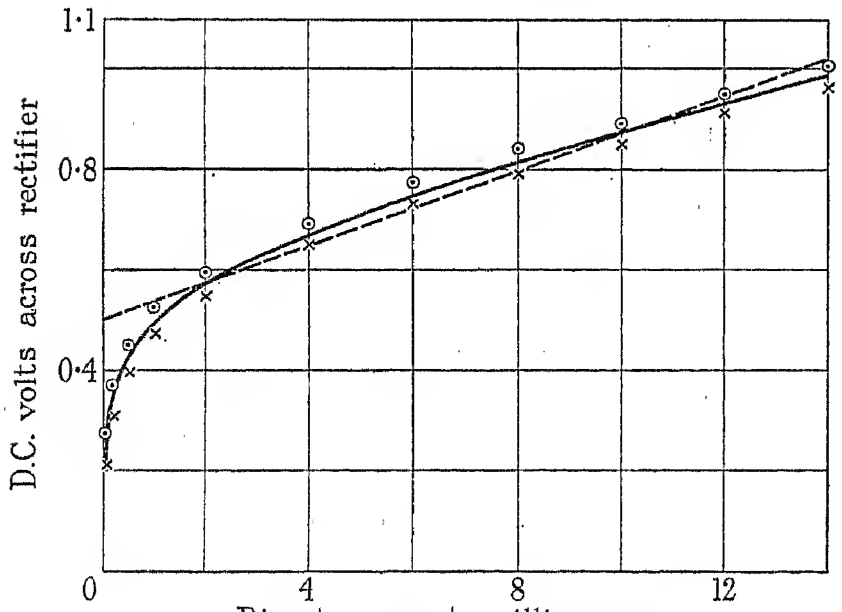


FIG. 4.

× 10-mA rectifier No. 1.
⊙ 10-mA rectifier No. 2.

rectifier resistance. From data obtained with direct current (Fig. 4), it is possible to construct the wave-forms of current due to sinusoidal voltages for different series resistances, and from these curves to calculate the mean and the effective values of the current. In the 10-mA rectifier under consideration, the resistance in the "reverse" direction for the same range of voltage varied between about 20 000 and 80 000 ohms, while the corresponding values for the 1-mA rectifiers were $0 \cdot 3$ to $0 \cdot 6$ megohm. Consequently the "reverse" current under these conditions is negligible compared with the "forward" current. Curves deduced in this way for the 10-mA rectifier are shown in Fig. 5; thus curve A gives the current wave-form when there is a resistance of $17 \cdot 3$ ohms in series. Its mean value is $3 \cdot 62$ mA, while the effective value is $4 \cdot 87$ mA. If the rectifier instrument had been calibrated with sinusoidal current, the scale reading would have been $4 \cdot 02$ mA (effective); hence the true effective value of the current would be $21 \cdot 1$ per cent higher than the indicated value. For curves B and C the corresponding errors are $14 \cdot 4$ and $2 \cdot 3$ per cent respectively. The results obtained from graphs similar to those of Fig. 5 are indicated by circles in Figs. 2 and 3, and are in close agreement with the experimental results.

The graphical method just described is very laborious,

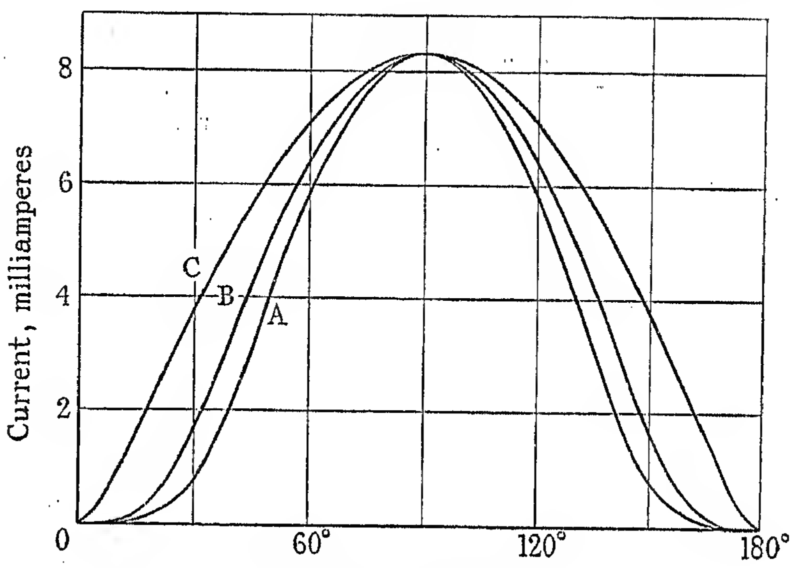


FIG. 5.—10-mA rectifier.

A. 17·3 ohms in series.
B. 57·3 ohms in series.
C. 617·3 ohms in series.

and it was felt that it would be more satisfactory if a comparatively simple mathematical expression could be evolved to give the desired result. Unfortunately, no such formula could be discovered. On plotting the resistance of the rectifier against the current on logarithmic scales, it was found that over a very wide range of current the resistance of the 10-, 5-, and 1-mA rectifiers varied approximately as $(\text{Current})^{-0 \cdot 8}$. Hence, if a resistance R be connected in series, and V_m be the maximum value of the applied sinusoidal voltage, the instantaneous value of the current is given by

$$i = \frac{V_m \sin \theta}{R + ki^{-0 \cdot 8}}$$

where k is a constant for a given rectifier. The only method of deriving the relationship between the mean and the effective values of this current wave appears to be to plot curves similar to those of Fig. 5 and then to calculate the mean and the effective values. This method is just as laborious as the one detailed above and is less satisfactory owing to the index of i being an approximation only.

Another alternative is to assume the relationship

between the direct current through the rectifier and the d.c. voltage across it to be of the form

$$v = c + bi \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

In Fig. 4, the crosses and circles represent the potential differences for various currents through the two 10-mA rectifiers, and the curve represents the mean potential difference. The dotted line, on the other hand, has been drawn to represent Equation (1): obviously some latitude is permissible in the position of such a line.

If R be the resistance in series with the rectifier across a sinusoidal voltage, the instantaneous value of the current is given by

$$i = \frac{V_m \sin\theta - c}{R + b}$$

It can be shown that the average value of such a current wave is

$$I_a = \frac{2}{\pi(R + b)}\left\{V_m \cos\alpha - c\left(\frac{\pi}{2} - \alpha\right)\right\} \quad . \quad (2)$$

where $\sin\alpha = c/V_m$; and the effective value is:—

$$I = \frac{1}{R + b}\sqrt{\left[\frac{2}{\pi}\left\{\frac{V_m^2}{4}(\pi - 2\alpha + \sin 2\alpha) - 2cV_m\cos\alpha + c^2\left(\frac{\pi}{2} - \alpha\right)\right\}\right]} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Values of (Form factor)/1·11, calculated from these expressions for the 10-mA and the 1-mA rectifiers with various series resistances, are represented by crosses in Figs. 2 and 3. It is seen, however, that these points

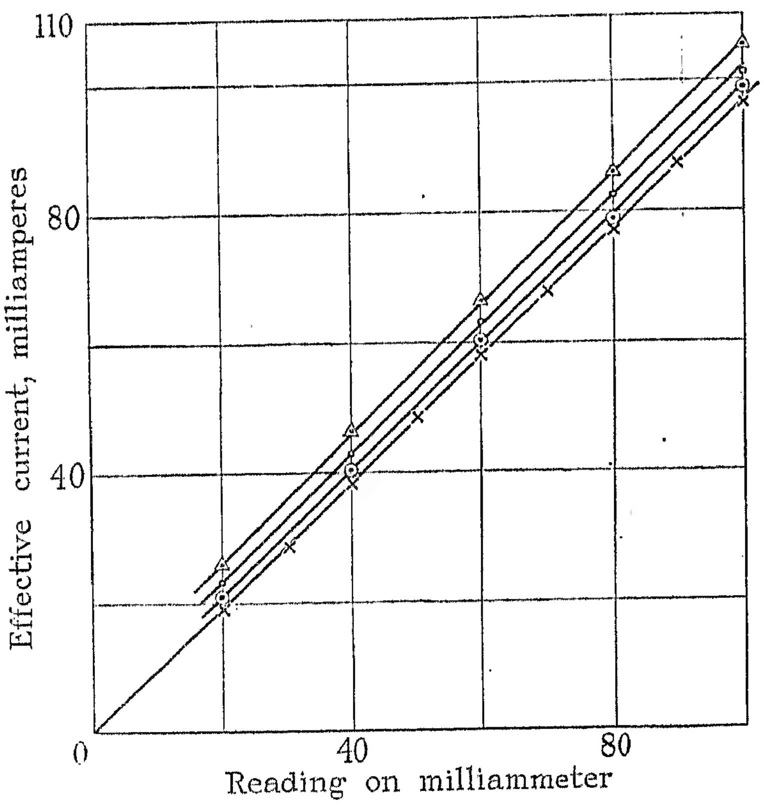


FIG. 6.

× 760 ohms in series.
⊙ 55 ohms in series.
o 15 ohms in series.
△ 5 ohms in series.

may differ quite considerably from the experimental values, especially for small currents; consequently this method cannot be recommended except for giving quickly a rough estimate of the above ratio.

A test similar to that described in connection with Fig. 1 was made on an unshunted 100-mA rectifier instrument supplied by well-known manufacturers. The instrument had been calibrated to read the effective

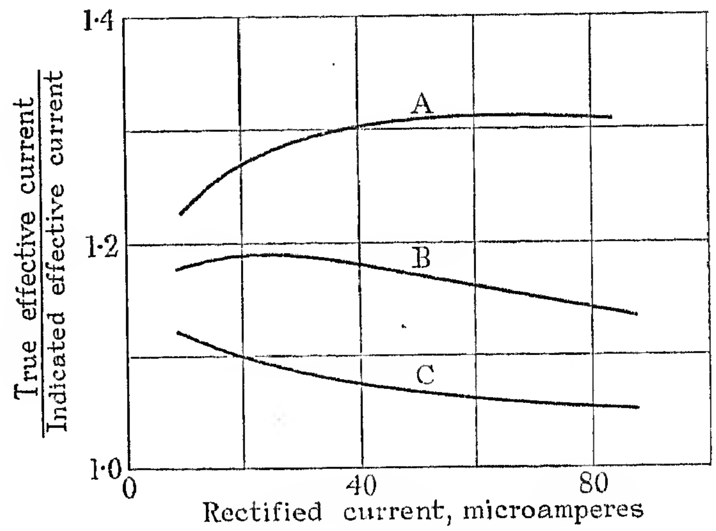


FIG. 7.—1-mA rectifiers.

A. No resistance in series.
B. 2 000 ohms in series.
C. 10 000 ohms in series.

value of the current. The results for certain series resistances are plotted in Fig. 6. It would appear from these curves that the instrument had been calibrated off a constant-voltage a.c. supply with a variable resistance in series; thus the resistance appears to have been about 30 ohms at 100 mA, 55 ohms at 60 mA, etc. It will be seen that if the instrument has been calibrated with a sinusoidal current, there will be considerable error if it is used in a circuit having a low resistance.

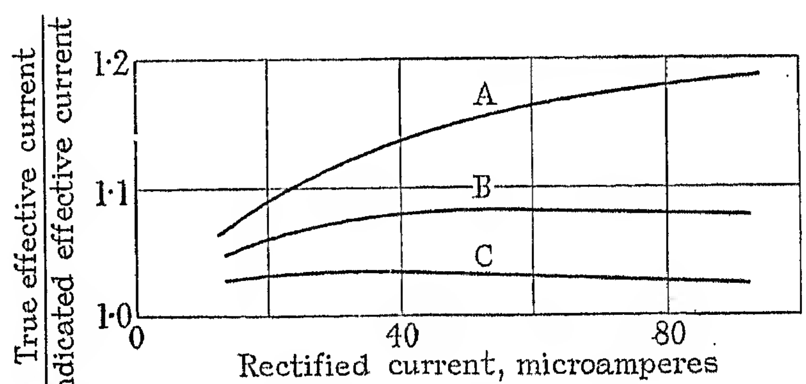


FIG. 8.—10-mA rectifiers.

A. No resistance in series.
B. 2 000 ohms in series.
C. 10 000 ohms in series.

In view of the possibility of using a microammeter with a rectifier unit, it was felt desirable to investigate the error that may then occur owing to the variation of the rectifier resistance. Unfortunately, no combination of thermo-junction and galvanometer was available for measuring the effective value of such small currents. In view, however, of the good agreement on the milliampere range between the experimental values and those calculated from graphs based upon d.c. tests, it seemed permissible to employ the same method on the microampere range, especially for the 1-mA rectifiers, since the "reverse" resistance of these units is many times

greater than the "forward" resistance, even with 1 μA. The results derived in this manner are given in Figs. 7 and 8, the values in each case being the mean of those for two similar units. A comparison of the two figures shows that, for a given external resistance, the error due to the variation of the rectifier resistance for the 10-mA unit is practically half that for the 1-mA unit, on account of the lower resistance of the former. The ratio of rectification is admittedly higher for the 1-mA unit, but tests made with a very high resistance in series with the rectifier showed this advantage to be appreciable only for currents below about 10 μA.

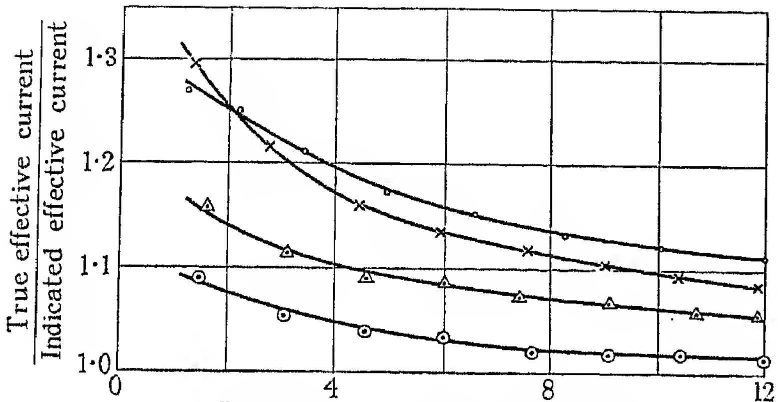


FIG. 9.—10-mA rectifier No. 1.

o R = 17·3 ohms.
△ R = 102·3 ohms.
⊙ X_L = 97 ohms; R = 17·5 ohms.
× X_C = 97·5 ohms; R = 17·3 ohms.

In the above discussion, non-inductive resistances only are considered. It was felt desirable to compare the effect of inductance and of capacitance with that of resistance. Fig. 9 shows the results obtained on a 10-mA rectifier with various types of impedances of about 100 ohms. It was anticipated that capacitive reactance would accentuate the error and that inductive reactance would reduce it. This is confirmed by Fig. 9. It was not found possible, however, to derive a mathematical expression that would take all the various factors into account with any degree of accuracy.

(3) EQUIVALENT RESISTANCE OF A RECTIFIER.

It is occasionally necessary (see page 460) to know the equivalent resistance of a rectifier; and this section

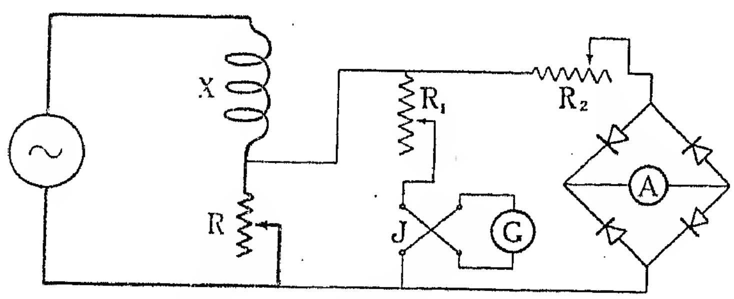


FIG. 10.

is devoted to a comparison of the values determined experimentally by different methods. One of the 10-mA rectifier units and a vacuo-junction J were connected as shown in Fig. 10. The alternating current through the air-choke X was large compared with the currents taken by the instruments, so that the potential difference across R remained sinusoidal throughout the test. With $R_2 = 0$, R was adjusted to give a certain reading, say 3 mA, on A, and R_1 was adjusted to give a convenient reading on G. R was then varied so as to quadruple the reading on G, i.e. to double the effective value of the applied voltage, and R_2 was adjusted to bring the reading on A back to the original value of 3 mA. The voltage was increased to 4 times the original value and the value of R_2 again noted. This series of readings was repeated for different currents through A.

Let r be the effective resistance of the rectifier unit (including the 5 ohms of the moving-coil instrument), and let r_1 and r_2 be the respective values of R_2 for effective voltages of 2 and 4 times the original value.

Then

$$(r + r_1)/r = 2; \ \therefore r = r_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and

$$(r + r_2)/r = 4; \ \therefore r = \tfrac{1}{3}r_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Or, alternatively,

$$(r + r_1)/(r + r_2) = \tfrac{1}{2}; \ \therefore r = r_2 - 2r_1 \quad . \quad (6)$$

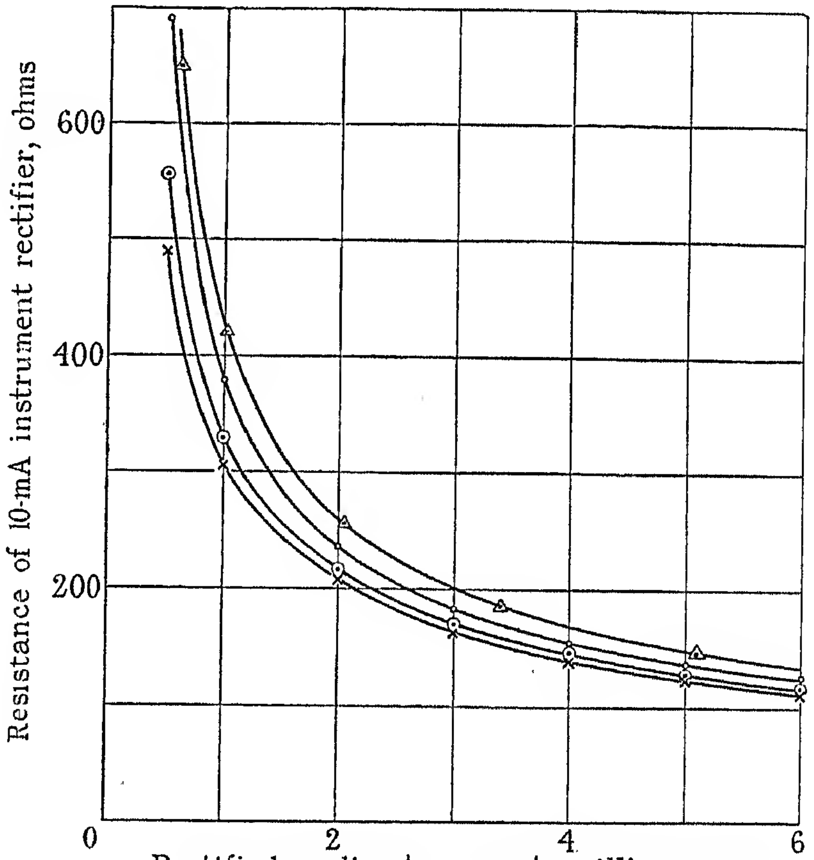


FIG. 11.

× From Equation (4).
⊙ From Equation (5).
o From Equation (6).
△ From test with direct current.

Fig. 11 shows the results given by the above expressions for the 10-mA rectifier. It also shows the resistance values calculated from tests with direct current, readings being taken with the battery connected first one way and then reversed. It will be seen that owing to the distortion of the current wave when there is little or no resistance in series with the rectifier, the effective resistance calculated from such readings may be considerably smaller than that deduced from results obtained with an almost undistorted wave; and, further, that the latter values do not differ greatly from those derived from d.c. tests.

(4) Effect of Series Resistance in Rectifiers Used with Voltmeters.

The effect of different series resistances upon the relationship between the rectified current and the applied voltage was investigated by connecting the rectifier in series with a variable resistance R_2, as shown in Fig. 10. The combination of R_1 and the vacuo-junction J was used as a r.m.s. voltmeter. Sets of readings of the rectified current and of the potential difference across R were taken for given values of R_2.

In general, the relationship between the rectified current and the applied voltage may be written in the form $I = kV^x$. By plotting graphs of $\log I$ against $\log V$, it was found that the relationship was linear over considerable ranges. The latter, together with the corresponding values of x, are given in the Table.

The indices given in the Table are almost identical with those obtained from the voltages and the mean currents of curves similar to those of Fig. 5; hence the departure of the exponent from unity must be due to the distortion of the current wave caused by the variation of the rectifier resistance. For very low series resistances, it is seen that the exponent may approach or even exceed 4; consequently, if such a rectifier circuit be connected across a shunt, the scale† is extremely cramped at the lower end—an undesirable feature, apart from any question of temperature error.

If the exponent of the voltage remains in the vicinity of 2 over a large range, it follows that the instrument should be almost independent of wave-form. This was confirmed by tests made with voltages of various wave-forms, and a case where advantage is taken of this characteristic is given on page 462.

(5) Effect of Wave-Form with a Rectifier Instrument Having a Linear Characteristic.

When the rectifiers referred to in the previous sections are working on the milliampere range with considerable resistance in series, it will be seen from the Table that the instrument reading is almost proportional to the mean value of the applied voltage. When used as an ammeter over the same range of rectified current, the "reverse" current is negligible compared with the "forward" current; consequently the reading is proportional to the mean current. One condition, however, must be satisfied for both the ammeter and the voltmeter, namely that the wave must not pass through zero more than twice per cycle. When such instruments are calibrated to read the effective value of a sine wave, they are not likely to be accurate for any other wave-form.

Suppose the current to be represented by

$$i = I_1 \sin \theta + I_n \sin (n\theta - \alpha)$$

where I_1 = amplitude of the fundamental; I_n = amplitude of the nth harmonic; and α = lag of the harmonic behind the fundamental in terms of the angular scale of the harmonic.

Table.*

Type of rectifier	Range of rectified current	R_2	x	Range of rectified current	R_2	x
	mA	ohms		μA	ohms	
1-mA	0·05 — 0·5	0	4·7	2 — 100	0	4·6
	0·5 — 1·5	0	3·1	2 — 30	6 600	2·4
	0·05 — 0·5	60	4·1	30 — 100	6 600	1·6
	0·5 — 1·5	60	2·7	2 — 10	60 000	1·5
	0·15 — 0·5	600	2·3	10 — 100	60 000	1·1
	0·5 — 1·5	600	1·56			
	0·1 — 0·7	2 000	1·47			
	0·7 — 1·5	2 000	1·2			
	0·1 — 0·7	7 000	1·2			
	0·7 — 1·5	7 000	1·01			
10-mA	0·1 — 1·5	0	4·15	3 — 20	0	2·8
	1·5 — 6·5	0	3·0	20 — 100	0	3·5
	6·5 — 13	0	2·0	3 — 40	6 600	1·8
	0·1 — 1·5	60	3·25	40 — 100	6 600	1·3
	1·5 — 4	60	2·2	2 — 40	60 000	1·4
	4 — 14	60	1·6	40 — 100	60 000	1·1
	0·2 — 2	600	1·35			
	2 — 12	600	1·12			
	0·3 — 6	2 000	1·08			

* Resistance of milliammeter = 5 ohms, and of microammeter = 34·4 ohms.

† For curves showing the effect of shunts of various resistances see E. H. W. Banner: *Electrical Review*, 1929, vol. 104, p. 823.

It can be shown that the average value of the current over half a cycle is

$$I_a = \frac{2}{\pi}\left\{I_1 + \frac{I_n \cos a}{n}\right\} \text{ when } n \text{ is odd,}$$

and $$I_a = \frac{2}{\pi} I_1 \text{ when } n \text{ is even.}$$

It is therefore obvious that the true mean ammeter and voltmeter neglect all even harmonics. For moving-coil instruments used with such rectifiers as the copper-oxide type, this is not true if the wave passes through zero more than twice per cycle.

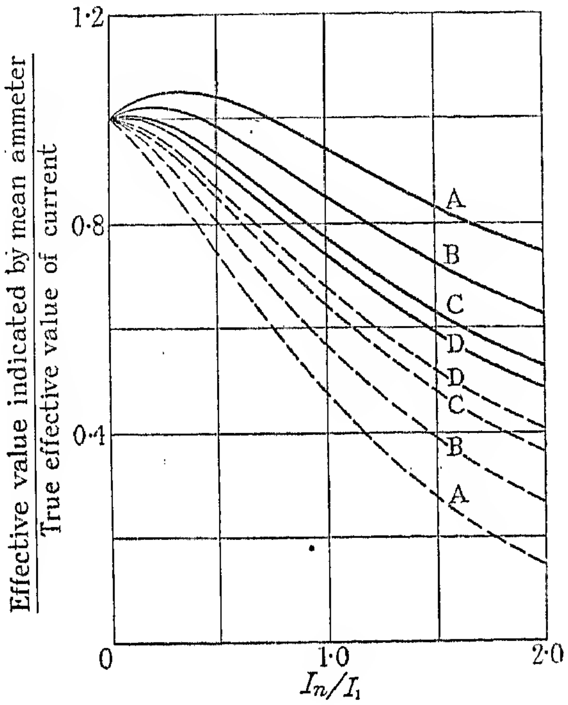


FIG. 12.

A. 3rd harmonic. B. 5th harmonic.
C. 11th harmonic. D. 23rd harmonic.

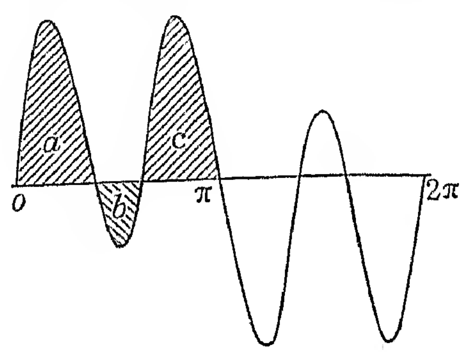


FIG. 13.

With odd harmonics, the average value must lie between the limits $(2/\pi)(I_1 \pm I_n/n)$, and in the following discussion only these limits will be considered. Hence,

$$\frac{\text{Reading on mean ammeter with harmonic present}}{\text{Reading on mean ammeter with fundamental only}} = 1 \pm \frac{I_n}{nI_1}$$

But

$$\frac{\text{Effective value of current with harmonic present}}{\text{Effective value of current with fundamental only}} = \sqrt{\left[1 + \left(\frac{I_n}{I_1}\right)^2\right]}$$

Since the instrument is assumed to be calibrated to read the effective value of sinusoidal waves,

$$\frac{\text{Effective value indicated by mean ammeter}}{\text{True effective value of current}} = \frac{1 \pm (I_n/nI_1)}{\sqrt{[1 + (I_n/I_1)^2]}} \quad . \quad (7)$$

Values calculated from this expression for different frequencies and amplitudes have been plotted in Fig. 12, the full lines being for the positive sign and the dotted lines for the negative sign.

If a moving-coil instrument be used in conjunction with a synchronous commutator, the reading is given by Equation (7) however large the amplitude of the harmonic; but where a unidirectional rectifier such as the copper-oxide type is used this expression only applies to wave-forms which do not cut the axis more than twice per cycle. There are circumstances, such as those referred to in Section (6), where the amplitude of the harmonic may be so large that the wave does cut the axis at more than two points per cycle. Fig. 13 gives the resultant curve of a fundamental and a third harmonic having twice the amplitude of the former. If a, b, and c be the respective areas indicated in Fig. 13, the average ordinate over half a cycle is $(a + c - b)/\pi$. If such a wave of current be passed through a moving-coil instrument used in conjunction with a full-wave copper-oxide rectifier, however, the average torque is proportional to $(a + c + b)/\pi$. Hence

$$\frac{\text{Mean value indicated on instrument}}{\text{True mean value}} = \frac{a + c + b}{a + c - b} = 1 + \frac{2b}{a + c - b} = 1 + \frac{2 \times \text{Negative areas over } \frac{1}{2}\text{-cycle}}{\text{Algebraic sum of areas over } \frac{1}{2}\text{-cycle}}$$

for the general case of any odd harmonic.

Since the instrument is assumed to be calibrated in terms of the effective values of sinusoidal waves,

$$\frac{\text{Effective value indicated by rectifier instrument}}{\text{True effective value}}$$

$$= \frac{\text{Mean value indicated by instrument}}{\text{True mean value}} \times \frac{\text{Effective value indicated by true mean ammeter}}{\text{True effective value}}$$

$$= \left(1 + \frac{2 \times \text{Negative areas over } \frac{1}{2}\text{-cycle}}{\text{Algebraic sum of areas over } \frac{1}{2}\text{-cycle}}\right) \times \frac{1 \pm I_n/nI_1}{\sqrt{[1 + (I_n/I_1)^2]}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

By drawing curves of fundamentals and of various harmonics of different amplitude and frequency, the required negative areas were measured with a planimeter; and the ratios calculated from Equation (8) are given in Fig. 14, the left-hand set of curves being for the positive sign. The corresponding values were also determined for the 7th and 17th harmonics, and were found to be within about 1 per cent of those for the 11th and 23rd harmonics. A comparison of Figs. 12 and 14 shows that when the current wave cuts the axis more than twice per cycle, the reading given by an instrument used with a rectifier of the copper-oxide type is generally much nearer the true effective value than that given by an instrument dependent upon the true mean value of the current. In fact, except for a certain range and phase of the 3rd harmonic, the rectifier instrument reads the effective value within 10 per cent for any shape of wave. This behaviour might be found useful in dealing with telephonic currents.

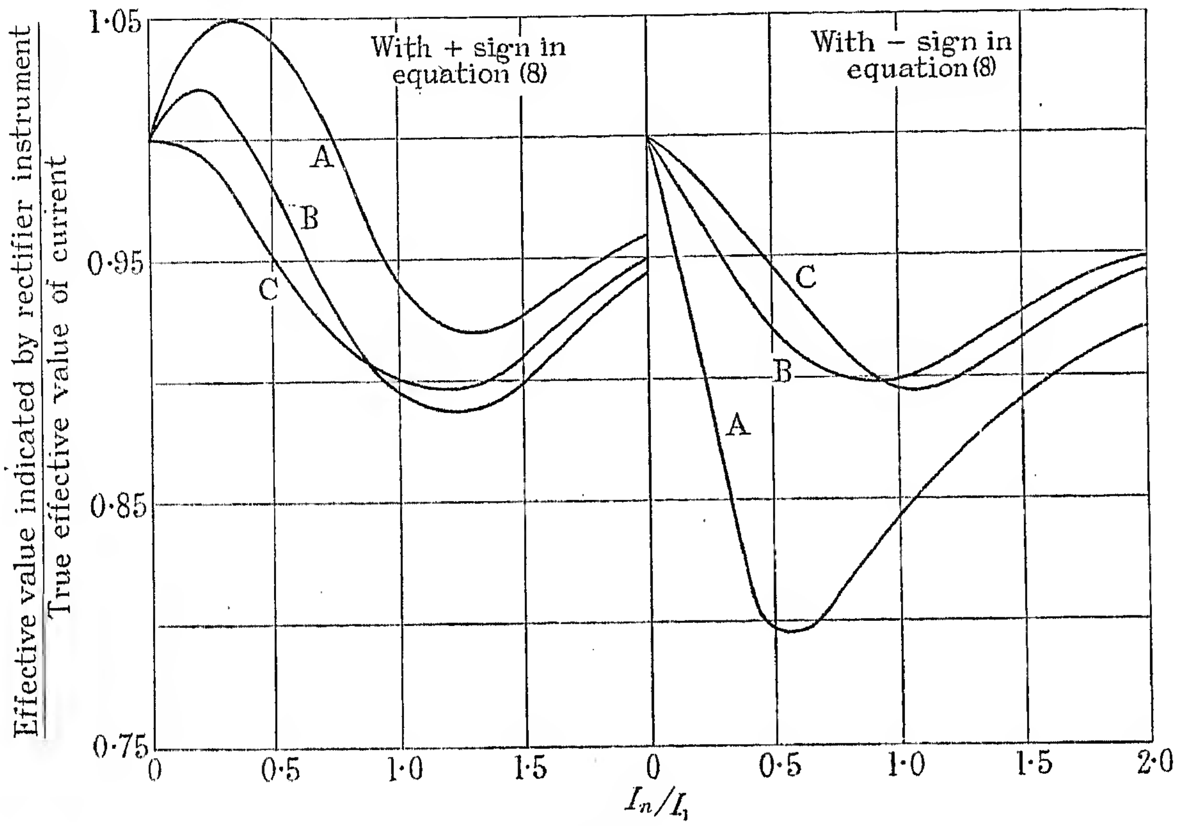


FIG. 14.

A. 3rd harmonic. B. 5th harmonic. C. 11th and 23rd harmonics.

(6) RECTIFIER INSTRUMENT IN CONJUNCTION WITH A CURRENT TRANSFORMER.

The disadvantage of using a shunted rectifier has already been referred to in Section (4). An alternative method of increasing the range of a rectifier ammeter or of enabling it to be used on a high-voltage circuit is to introduce a current transformer. Obviously there is no advantage in substituting a copper-oxide rectifier and a d.c. milliammeter for the usual a.c. ammeter unless there is a gain in sensitivity or in accuracy. Actually, the sensitivity is far greater than that of the usual arrangement, but larger errors are liable to be present.

If I_1 and I_2 be the primary and secondary currents respectively of a current transformer, then, for sinusoidal waves,

$$2\pi f M I_1 = I_2 \sqrt{[R^2 + (2\pi f L_2)^2]}$$

where M is the mutual inductance, L_2 and R are the self-inductance and resistance respectively of the secondary circuit, and f is the frequency. If T_1 and T_2 be the number of primary and secondary turns respectively, and if the leakage flux be negligibly small (as in the case of a toroid having an iron core), then

$$\frac{I_1}{I_2} = \frac{L_2}{M}\sqrt{\left[1 + \left(\frac{R}{2\pi f L_2}\right)^2\right]}$$
$$= \frac{T_2}{T_1}\sqrt{\left[1 + \left(\frac{R}{2\pi f L_2}\right)^2\right]} \quad . \quad . \quad . \quad (9)$$

Since the resistance of the rectifier varies with the current, it follows from Equation (9) that the ratio I_1/I_2 can be rendered independent of the current only by one or other of the following methods.

(*a*) If the reactance be made very large compared with the resistance, then $I_1/I_2 = T_2/T_1$. For frequencies of the order of 50, the reactance of the secondary of an ordinary commercial transformer is far too small. A secondary having a comparatively large inductance was made by winding a ring of 30 mumetal stampings, each $7\frac{1}{16}$ in. × $6\frac{1}{16}$ in. × 0·015 in., with 670 turns of No. 30 S.W.G. copper wire, the primary being a single turn. A 10-mA rectifier was connected directly across the secondary, and the results plotted in Fig. 15 were obtained at 50 cycles per sec. with the following loads:—(*a*) air-choke, (*b*) a 1-kVA 110-V transformer with its secondary on open circuit, (*c*) 96 μF, and (*d*) 22 μF. Oscillograms of representative currents taken by the

transformer and the two capacitance loads are given in Fig. 16. The effect of varying the frequency between 30 and 60 cycles per sec., with sinusoidal primary currents, is shown in Fig. 17.

For a sinusoidal current of 8 A at 50 cycles per sec., it was estimated that the maximum flux density in the mumetal was about 450 lines per cm^2; and, from separate tests on the mumetal stampings, the permeability for

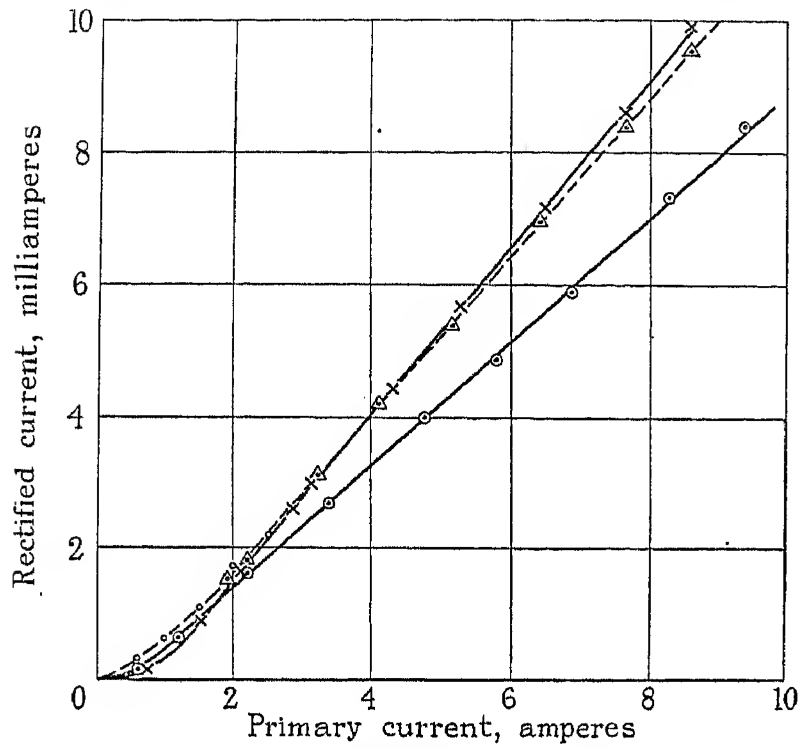


FIG. 15.
× Air-core choke.
⊙ Closed-iron-core choke.
△ 96 μF.
o 22 μF.

flux densities up to this value was found to be roughly 7 000; the corresponding value of $2\pi f L_2$ being 343 ohms. The values of the ratio (I_1 at 30 cycles per sec.)/(I_1 at 60 cycles per sec.) for given rectified currents were calculated by means of Equation (9) on the basis of this reactance and of the equivalent resistances given in Fig. 11; the values thus obtained were in close agreement with those derived from Fig. 17 over the range 1 to 10 mA; for example, for 4 mA the calculated ratio was 1·153, whereas the experimental ratio was 1·161.

The temperature coefficient of resistance of the copper-

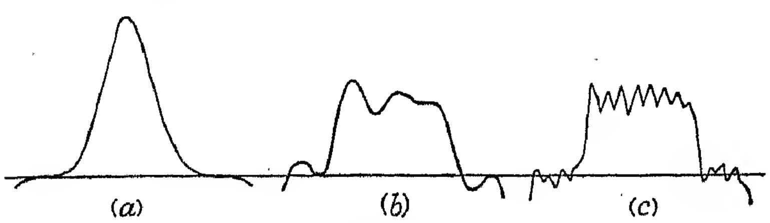


FIG. 16.
(a) 7·7 A. (b) 96 μF, 4 A. (c) 22 μF, 1·7 A.

oxide rectifier in the "forward" direction is about 1 to 1·2 per cent per deg. C., and its influence upon the rectified current was checked both by Equation (9) and also experimentally by inserting different resistances in series with the secondary circuit. These two sets of results were also in close agreement and indicated that the normal temperature variation of a room would not introduce an appreciable error. For instance, for a given primary current and a rectified current of about 4 mA, 8 per cent increase of the rectifier resistance reduced the ammeter reading by nearly 2 per cent; and the larger the current the smaller was the error.

These experiments suffice to prove that when the rectifier is used under these conditions the effects of frequency and of secondary impedance can be estimated with considerable accuracy, and that the ratio I_1/I_2 can be rendered practically independent of frequency and temperature variations by making L_2 sufficiently large.

(*b*) If R be made large compared with $2\pi f L_2$, e.g. if a comparatively large resistance be inserted in series with the rectifier and the secondary be wound with relatively few turns, then Equation (9) becomes

$$\frac{I_1}{I_2} = \frac{T_2}{T_1} \times \frac{R}{2\pi f L_2}$$

The effect of the variation in the permeability of the

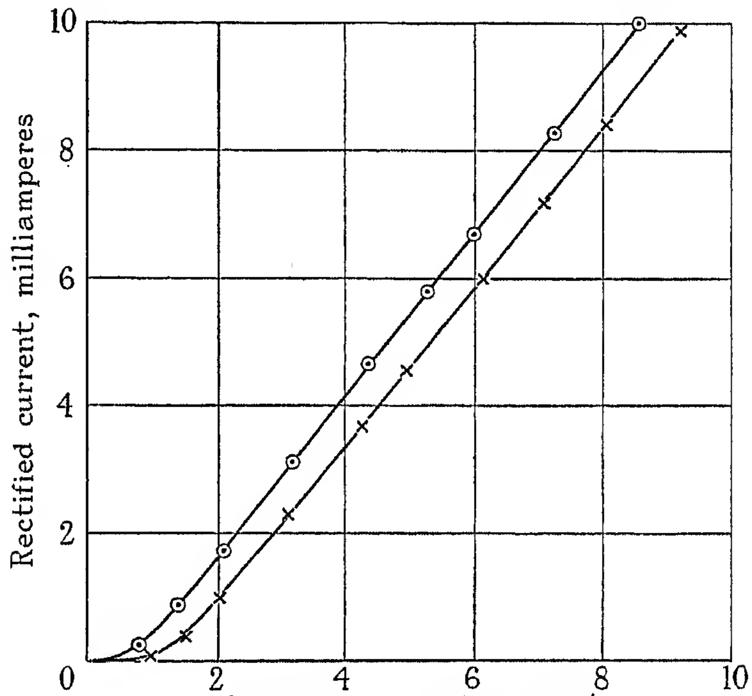


FIG. 17.
× 30 cycles per sec.
⊙ 60 cycles per sec.
Load: air-core choke.

iron ring may be practically eliminated by introducing a short gap into the magnetic circuit. Consequently, for a given frequency and assuming sinusoidal primary currents, the ratio I_1/I_2 is practically constant. Unfortunately, the presence of small high-frequency ripples in the primary current introduces unexpectedly large errors. The transformer is now operating almost as if it were on open circuit, and the combination of rectifier, high resistance, and milliammeter, is acting as a voltmeter. Fig. 18 shows the results obtained with a current transformer consisting of a temporary primary winding of 12 turns around a choke coil having a short air-gap in its magnetic circuit, the original coil being used as the secondary. The loads were similar to those referred to in connection with Fig. 15, and the secondary voltage was measured with a milliammeter and a 10-mA rectifier having 1 000 ohms in series and calibrated on a sinusoidal voltage. The primary current was measured with a hot-wire ammeter.

Modern turbo-alternators are designed with their

voltage wave as sinusoidal as possible; but even the imperceptible high-frequency tooth ripples* inevitably present may give rise to comparatively large harmonics in the current taken by a lightly-loaded cable or other capacitive load. Further, the wave-form of the current taken by a motor is dependent upon the difference between the wave-forms of the applied voltage and of the back e.m.f., and since the latter is seldom sinusoidal, especially in synchronous motors, the current wave may be expected to have appreciable harmonics† present. It is therefore of practical interest to consider the effect

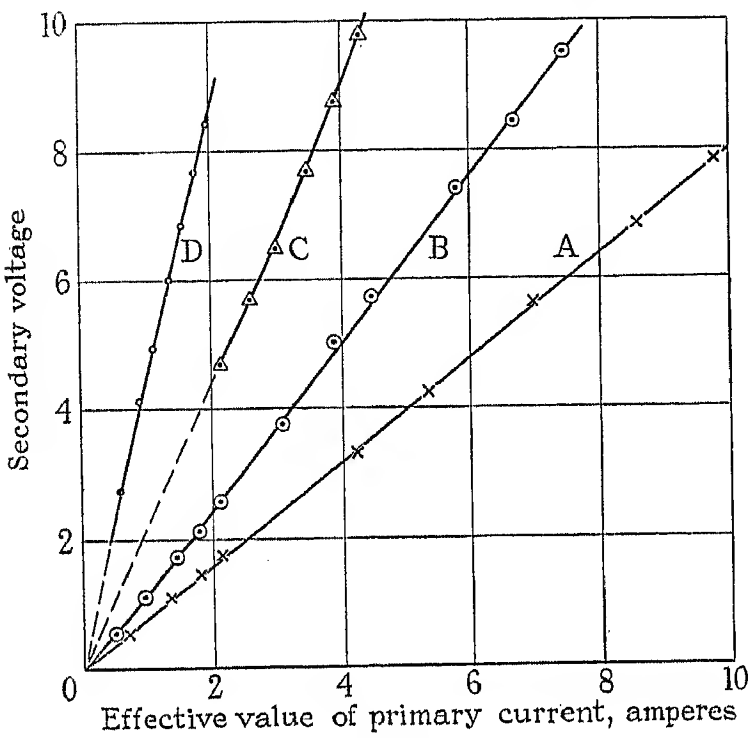


FIG. 18.

A. Air-core choke.
B. Closed-iron-core choke.
C. 96 μF.
D. 22 μF.

of harmonics upon the reading given by a rectifier voltmeter connected across the secondary of a current transformer.

Suppose the primary current to be represented by

$$i = I_1 \sin \theta + I_n \sin (n\theta - \alpha)$$

If the reluctance of the magnetic circuit of the current transformer be assumed constant, then the e.m.f. induced in the secondary is represented by

$$e = k\left\{I_1 \sin (\theta - 90^\circ) + nI_n \sin (n\theta - \alpha - 90^\circ)\right\} \quad . \ (10)$$

where k is a constant for a given transformer; and the mean value of this voltage lies between the limits $(2k/\pi)(I_1 \pm I_n)$, as explained in Section (5). As before, only these limiting values will be dealt with below. Also, for simplicity, the series resistance will be assumed large compared with that of the rectifier, so that the relationship between the voltage and the rectified current may be taken as linear.

Since the voltmeter will have been calibrated to read the effective value of sine waves, for waves which do not cut the axis more than twice per cycle we have

$$\frac{\text{Actual reading on voltmeter}}{\text{Reading with fundamental only}} = 1 \pm \frac{I_n}{I_1}$$

It will be evident from Equation (10) that if the primary current contains a comparatively small harmonic of a high order, it will appear as a very large harmonic in

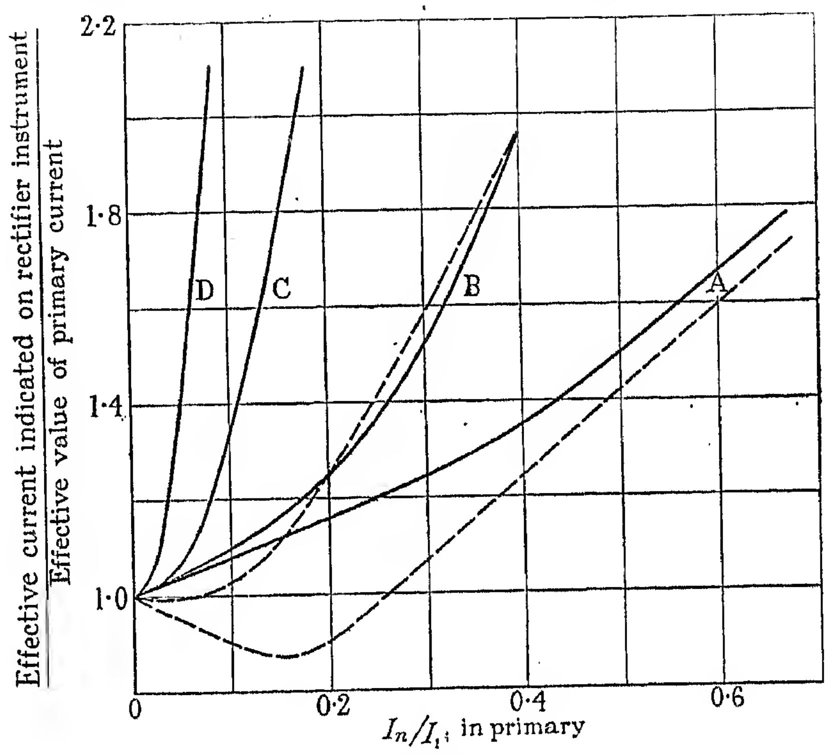


FIG. 19.

A. 3rd harmonic.
B. 5th harmonic.
C. 11th harmonic.
D. 23rd harmonic.

the secondary voltage, so that the latter may be cutting the axis more than twice per cycle. If the secondary instrument has been calibrated with a sinusoidal primary current, then, referring to Fig. 13, we have:—

$$\frac{\text{Mean current through rectifier instrument}}{\text{True mean current}} = 1 + \frac{2b}{a+c-b}$$

Therefore

$$\frac{\text{Mean value indicated on rectifier instrument}}{\text{Mean value indicated with fundamental only}} = \left(1 + \frac{2b}{a+c-b}\right)\left(1 \pm \frac{I_n}{I_1}\right)$$

Since the rectifier instrument would normally be calibrated with sinusoidal waves to read directly the effective value of the primary current, we have, for the general case

$$\frac{\text{Effective current indicated on rectifier instrument}}{\text{Effective value of primary current}}$$
$$= \left(1 + \frac{2 \times \text{Negative areas of secondary wave over } \frac{1}{2}\text{-cycle}}{\text{Algebraic sum of areas over } \frac{1}{2}\text{-cycle}}\right) \times \frac{1 \pm I_n/I_1}{\sqrt{[1 + (I_n/I_1)^2]}} \quad . \ (11)$$

* *Metropolitan-Vickers Gazette*, 1928, vol. 10, p. 301.
† See A. F. T. ATCHISON: "Some Properties of Alternators under Various Conditions of Load," *Journal I.E.E.*, 1904, vol. 33, p. 1062, where a large assortment of oscillograms will be found.

This ratio has been calculated for a number of harmonics and for various amplitudes of those harmonics; the results are given in Fig. 19. The full and the dotted lines are for the positive and negative signs respectively in Equation (11). For the 11th and 23rd harmonics, the two sets of values were practically identical. It will be seen that a comparatively small high-frequency harmonic in the primary current may cause considerable error in the reading given by the secondary instrument, thereby confirming the experimental results represented by Fig. 18.

(c) If the resistance R of the secondary circuit be made very small by connecting a comparatively low resistance

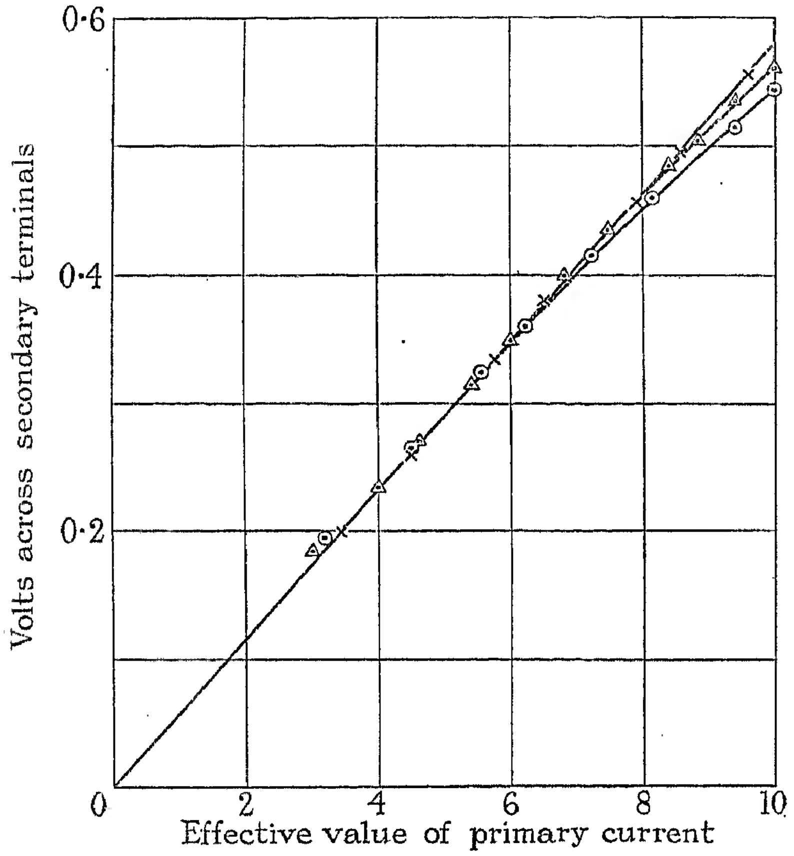


FIG. 20.

× Air-core choke.
⊙ Closed-iron-core choke.
Δ 96 μF.

across the secondary of an ordinary current transformer, the potential difference across the resistance can be measured by a rectifier voltmeter calibrated in terms of the primary current. The curves in Fig. 20 give the results obtained by connecting a resistance of 0·38 ohm across the secondary of a 30/5-ampere current transformer. The primary and secondary windings had 27 and 162 turns respectively. No measurable error was found for a frequency variation of 30 to 75 cycles per sec. In these tests the voltmeter consisted of a 10-mA rectifier in series with a resistance of 380 ohms. Tests were made with various secondary resistances up to 5·2 ohms, and the effects of wave-form and of frequency were found to increase gradually with increase of resistance. Oscillograms of the secondary potential difference indicated that within this range there was only a very slight change in the wave-form for a given current in the primary. The wave-form error is almost zero in Fig. 20, apparently owing to the rectifier voltmeter having a comparatively small resistance in series, so that its deflecting torque is approximately proportional to the square of the voltage, as explained in Section (4).

(7) CONCLUSIONS.

(1) The introduction of a rectifier into a circuit of comparatively low resistance distorts the current wave and causes a rectifier ammeter calibrated with a sine wave to read low. The lower the resistance the greater is the error; and, for a given resistance, the smaller the current the greater, in general, is the error.

(2) Owing to its lower resistance, a 10-mA rectifier is generally much more suitable than a 1-mA unit for measuring currents of the order of microamperes.

(3) Inductive reactance reduces the distortion of the current and thus reduces the error in a rectifier ammeter calibrated with sinusoidal current. Capacitive reactance, on the other hand, has the opposite effect.

(4) The value of the effective resistance of a rectifier is affected by the amount of resistance in series, being lower the smaller the resistance in series.

(5) The relationship between the rectified current and the voltage applied to a rectifier voltmeter depends upon the value of the series resistance. With comparatively low values of the latter the exponent of the voltage is approximately 2, and the instrument reads roughly the effective value of the alternating voltage independently of wave-form.

(6) When a rectifier instrument having a linear characteristic is used with a very badly distorted wave, its reading is generally much nearer the true effective value than that given by an instrument dependent upon the true mean value of the current. Except for a certain range and phase of the third harmonic, a rectifier instrument calibrated with a sine wave reads the effective value within 10 per cent for any shape of wave.

(7) When a rectifier instrument is used in conjunction with a current transformer, the ratio of primary to secondary current may be rendered practically independent of the current, frequency, and wave-form, either (a) by giving the secondary winding a very large self-inductance, or (b) by connecting a low resistance across the secondary terminals of a commercial current-transformer and using the rectifier instrument as a low-reading voltmeter to measure the potential difference across that resistance.

(8) If the rectifier be connected in series with a comparatively high resistance across a secondary of relatively low reactance, the rectified current is proportional to the frequency and is greatly affected by harmonics in the primary current; and the higher the frequency of the harmonic the greater is the error.

ACKNOWLEDGMENT.

The author desires to express his thanks to the Principal and the Governors of the Municipal Technical College, Brighton, for facilities to carry out the experimental work described in this paper.

[The discussion on this paper will be found on page 469.]

A DIRECT-READING FORM FACTOR METER.

By R. S. J. Spilsbury, B.Sc.(Eng.), Member.

[From the National Physical Laboratory.]

(*Paper first received 8th December*, 1933, *and in final form* 12*th January*, 1934; *read before the* Meter and Instrument Section 2*nd March*, 1934.)

Summary.

Methods at present in use for determining form factor are outlined, and their good and bad features are indicated. A description is given of a new instrument, incorporating a copper-oxide rectifier, which gives direct indications of the form factor. Test results show that the instrument is sensibly independent of variations of voltage, frequency, and temperature. The form factors of waves of varying shape are shown to be correctly indicated, except in the cases of re-entrant waves, and rectangular waves of a particular type. The effects of this last class of waves are discussed in an Appendix.

Introduction.

The determination of the form factor of an alternating voltage or current wave is of importance in several branches of electrotechnics; for example, in the testing of transformer steels, where the eddy-current losses are proportional to the square of the form factor, and in the calibration of rectifier instruments, which are scaled to indicate r.m.s values, though their indications are proportional to average values. Two methods of determining the quantity are in use. In the first, the wave-shape of the voltage or current concerned is recorded by an oscillograph, a number of ordinates of the wave are measured, and the form factor is calculated from the mean and r.m.s. ordinates. The advantages of this method are that it is simple and direct, and that it gives the wave-shape as well as the form factor. The disadvantages are that the oscillograph is costly; that the method is exceedingly slow and laborious, the measurements and calculations for a single result taking about half an hour; that the accuracy is low, the uncertainty being of the order of 1 per cent; and that the heavy current consumption (about 100 milliamperes) of oscillographs of the usual type is likely to cause a serious alteration in the form factor.

In the second method the r.m.s. value of the wave is measured by an electrometer, thermal instrument, or dynamometer, while the mean value is obtained by the use of a permanent-magnet moving-coil instrument, in conjunction with a synchronously-driven commutator-rectifier. The advantages of this method are that it is reasonably rapid; that the accuracy is high (of the order of 0·1 per cent); and that the current consumption of the two instruments can be made very small. The disadvantages are that the phase of reversal of the commutator has to be adjusted by trial, an operation usually requiring two observers; that the generator itself, or a synchronous motor free from hunting, must be available; and that considerable care is necessary to avoid troubles due to bad contact between the brushes and commutator.

Neither of the two methods mentioned lends itself to the construction of portable apparatus.

Principle of the Meter.

It appeared that the majority of the defects of the methods described could be avoided by the use of an instrument incorporating a copper-oxide rectifier, arranged to work at a constant r.m.s. voltage. Such an instrument would consist of a low-range dynamometer voltmeter, connected in parallel with a permanent-magnet moving-coil voltmeter with rectifier, means being provided to vary the voltage applied to the combination until the dynamometer indicates some standard value. When this adjustment has been made the indication of the

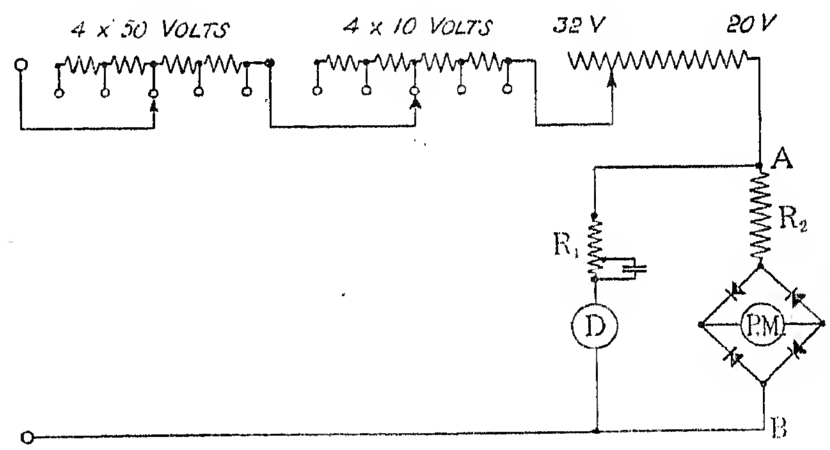


Fig. 1.

rectifier instrument will depend only upon the form factor of the applied voltage, and the instrument can be scaled to give this quantity directly. Fig. 1, to which more detailed reference will be made later, shows the principle of the instrument.

The difficulties met with in the design of the meter arise from the wide range of frequency over which the instruments must be accurate, and from the imperfections of the copper-oxide rectifier. As regards the first point, it is necessary to keep the power consumption of the meter low if it is to be of use in magnetic testing. The dynamometer must therefore be wound for a very low current, and so must have a relatively high inductance, while the available voltage will not be sufficiently high to allow a large "swamp" resistance to be used. Fortunately, however, the well-known device of connecting capacitance in parallel with a part of the swamp resistance allows this error to be reduced to a negligible

amount for fundamental frequencies up to at least 800 cycles per sec. The frequency characteristic of the rectifier instrument is satisfactory without compensation over this range.

The limitations due to the rectifier itself are more serious, and arise from the fact that the impedance of the copper-oxide device in the "forward" direction varies very greatly with the current rectified. For the particular type used in the instrument under discussion, the impedance at a current corresponding to full-scale reading on the associated instrument is approximately 1 500 ohms, while at one-twentieth of this current the impedance is approximately 15 000 ohms. In consequence, the curve of direct-current output against applied direct-current voltage for a rectifier in series with a fixed resistance is of the form shown in Fig. 2.

When an alternating voltage is applied to the rectifier circuit, therefore, the instantaneous output current is not exactly proportional to the instantaneous applied voltage: hence the mean output current during a half-

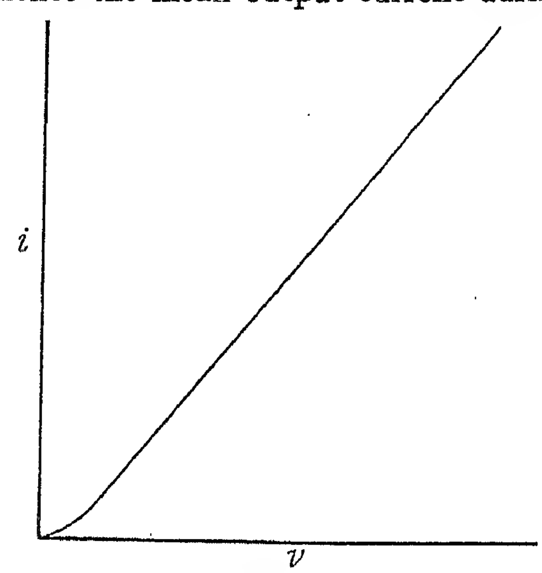


FIG. 2.

cycle will depend upon the wave-shape of the applied voltage, even when the mean value of the voltage wave is maintained constant.

The effect is not of importance in ammeters, etc., embodying rectifiers, because, as has been mentioned, such instruments are usually scaled to indicate r.m.s. values, and so have already a very large wave-form error due to variation of form factor, which renders them unsuitable for non-sinusoidal currents. In the present case, however, in which the instrument is specifically intended to be used on circuits of very varied wave-form, and in which the error due to form factor does not arise, the lack of proportionality in the response of the rectifier sets a low limit for the operating voltage of the meter.

Referring again to Fig. 2, the rectifier-circuit characteristic is seen to consist mainly of a straight line, not passing through the origin. The equation connecting the instantaneous output current i with the instantaneous applied voltage v for this part of the characteristic is $i = (v - b)/r$, where b is a constant, and r is a constant for a given value of the rectifier-circuit resistance. The lower portion of the characteristic is of curved form.

The effects of the constant b, and of the curvature near the zero, may be reduced by increasing the mean value of the applied voltage v. This value, which fixes the minimum working voltage of the instrument, must be so chosen that the mean output current of the rectifier is independent of wave-form, within the desired limits. This quantity is ascertained by measuring a number of equally-spaced ordinates of the wave-shape under consideration, determining the value of i for each of these values of v from the rectifier-circuit characteristic, and averaging the values of i. In this way the readings of the rectifier instrument for any two voltage waves of different shapes, but with equal mean values, may be compared.

The minimum working voltage of the meter depends upon the range of wave-shape over which the accuracy of indication must be maintained. For example, the wave-shape of the voltage applied to an instrument designed to work in conjunction with an iron-testing set would ordinarily vary within fairly narrow limits only, for a given value of form factor. In such a case, if an accuracy of 0·2 per cent were desired, the meter could be made to operate satisfactorily down to a voltage as low as 5 volts. The instrument to be described was, however, intended for use with voltages having any possible wave-form, with two exceptions to be discussed later. Calculations showed that if an accuracy of 0·2 per cent was to be maintained under these conditions the minimum working voltage was of the order of 20 volts.

In the preceding discussion no mention has been made of another characteristic of the rectifier, namely the leakage current through the two inactive elements of the bridge network. Such leakage is relatively much larger at low forward currents than at high ones, and gives rise, therefore, to wave-form errors of the same type as those due to variation of the forward impedance. The errors due to leakage are only about one-tenth as large as those which have been discussed, and in practice they are negligible: if, however, the constants of the rectifier are determined by measurements on the full-wave rectifying bridge, allowance is automatically made for such effects.

For voltages higher than the minimum value, resistance is added in series with the dynamometer-rectifier combination. The errors of the instrument will not be affected by this addition so long as the wave-form of the voltage across A–B (Fig. 1) is a true reproduction of the wave-form of the voltage at the instrument terminals. Since both branches of the circuit A–B are substantially free from reactance, no distortion due to this cause will occur. Distortion due to variation of the impedance of the rectifier is minimized by the presence of the swamp resistor R_2, which may have a resistance of about 40 000 ohms in a 20-volt instrument, and by the fact that the rectifier instrument circuit is shunted by the dynamometer circuit, which will ordinarily have an impedance less than one-tenth as great. Under these conditions the change of error due to added resistance is negligible.

Construction of the Meter.

With the foregoing considerations in view, an instrument was designed with the co-operation of the Cambridge Instrument Co., and was manufactured by that firm.

Various modifications of the original design have been made, and the form described here is the final one. The meter is contained in a wooden case 19 in. × 8 in. × 5 in., provided with a slip-hinged lid and a carrying strap. The case is divided into three parts, which contain respectively the voltage-adjusting rheostats, the dynamometer instrument, and the rectifier instrument: the details of the circuits are shown in Fig. 1. The rheostat compartment carries a panel on which are mounted two 5-position turning heads, controlling resistors suitable respectively for 50 volts per step and 10 volts per step, and a further head controlling a continuously variable resistor suitable for 12 volts. The minimum working voltage of the instruments being 20 volts, the meter is applicable to voltages of 20 to 272 volts by adjustment of the turning heads. The two heads controlling the larger resistors are provided with safety devices which prevent the lid from being closed unless the heads are set to their highest positions. The panel also carries a key which breaks the instrument circuits and so allows the zeros to be checked.

The central compartment of the case contains the dynamometer D, which is of the unipivot type, and gives its working deflection with a current of 10 milliamperes. As has been mentioned, a condenser C is shunted across a part of the series resistor R_1, in order to provide compensation for the change of impedance of the dynamometer with frequency.

Since the 20-volt rectifier instrnment has a negative temperature coefficient of sensitivity of the order of 0·05 per cent per deg. C., it is desirable to give the dynamometer instrument a similar temperature coefficient, and so reduce the effect of temperature on the form factor indication. The proportion of copper in the dynamometer circuit is insufficient to give the desired value, and a small amount of nickel wire is therefore included in R_1.

The instrument scale carries only two marks—the zero line, and a fiducial line near the top of the scale marked "Set Volts." When the pointer of the dynamometer has been brought to this line by adjustment of the rheostats, an r.m.s. voltage of 20 volts exists between the points A and B (Fig. 1).

The rectifier instrument, contained in the right-hand compartment, consists of a unipivot permanent-magnet moving-coil instrument (P.M. in Fig. 1) shunted to give full deflection for about 0·4 milliampere. It is fed by a Westinghouse instrument-type rectifier, rated at 1 milliampere maximum, through the series resistor R_2. The upper portion of the scale-plate carries a scale of form factor running from 1·320 to 1·000, the mean length of a scale division, representing 0·010, being 0·8 mm. This scale is placed on the rectifier instrument rather than on the dynamometer in order to allow the dynamometer to be used as a voltmeter without resetting the rheostats.

The calibration of the instrument was carried out on a supply whose wave-form was known to be closely sinusoidal. The dynamometer pointer having been brought to its fiducial mark, the position of the pointer of the rectifier instrument gave the scale point corresponding to a form factor of 1·111. Assuming that the rectifier response is independent of the applied wave-form, further scale points could then be determined by applying a voltage of $1 \cdot 111x/F$, where x is the voltage corresponding to "Set Volts" on the dynamometer, and F is the value of the form factor corresponding to the scale-point to be marked. Actually, small calculated corrections, of the order of 0·2 per cent, were made to the voltages so applied, in order to allow for the difference between the rectifier responses for a sine wave, and for the more peaked waves corresponding to higher form factors.

Performance of the Meter.

The completed instrument was tested by comparison with the permanently-installed apparatus of the Electrical Measurements Division of the Laboratory. This apparatus consists of a sensitive reflecting electrometer and a reflecting permanent-magnet moving-coil galvanometer: the latter instrument is used in conjunction with a rectifier of the synchronous-commutator type, mounted on the shaft of the main alternator. The form factor sensitivity of the apparatus is about 0·1 per cent, and the maximum error is probably about 0·2 per cent. In some cases this apparatus was connected to a tertiary winding of the transformer supplying the meter, while in others a resistance voltage-divider was employed. In all tests concerned with the effect of wave-form, and in a number of the other tests, a low-period Duddell oscillograph was employed to record the wave.

Scale Errors.

For these tests, waves containing harmonics of low order only were employed. Fig. 3 shows a typical wave of high form factor, while Fig. 4 shows one of low form factor. The errors at a frequency of 50 cycles per sec. and with an applied voltage of 100 volts are given in Table 1.

TABLE 1.

Meter reading	Percentage error
1·052	$-0\cdot_3$
1·119	$-0\cdot_1$
1·165	$+0\cdot_1$
1·205	$-0\cdot_1$
1·278	$+0\cdot_2$
1·313	$+0\cdot_1$

These figures of error indicate that the method of calibration already described is satisfactory, since they are of the same order as the experimental error arising in the comparison of the meter and standard. This point is of some importance, because the synchronous commutator is not very generally available, and it is therefore an advantage to be able to determine the scale points by other means.

Effect of Variation of Voltage.

The effect on the errors of the meter of variation of the applied voltage was determined at low and high

form factors, at a frequency of 50 cycles per sec. The results obtained are given in Table 2.

TABLE 2.

Change of voltage	Change of error (per cent)	
	At form factor 1·12	At form factor 1·31
volts		
100–24	+ 0·1	− 0·1
100–250	+ 0·1	0·0

These figures indicate that the errors of the meter are independent of the applied voltage, to the accuracy sought.

Effect of Variation of Frequency.

The effect of variation of frequency was investigated over the range 25–800 cycles per sec., at a voltage of 30 volts. The wave-shape employed for frequencies above 100 cycles per sec. was that shown in Fig. 8: this wave has a considerable harmonic content, though the form factor is low. The results obtained are given in Table 3: the values for frequencies above 100 cycles per sec. are slightly less reliable than those for lower frequencies.

TABLE 3.

Change of frequency	Change of error (per cent)	
	At form factor 1·11	At form factor 1·30
cycles per sec.		
50— 25	+ 0·1	− 0·1
50—100	0·0	− 0·1
50—300	0·0	
50—500	− 0·2	
50—800	+ 0·1	

These results indicate that the errors of the meter are independent of frequency, up to 800 cycles per sec.

Effect of Variation of Temperature.

The temperature coefficient of the errors of the meter was determined over the range + 15° C. to + 35° C., at two voltages and at various form factors. The changes of error caused by an increase in temperature of 20 degrees C. are given in Table 4.

TABLE 4.

Voltage	Form factor	Change of error
		per cent
30	1·1	− 0·1
30	1·2	+ 0·1
30	1·3	+ 0·1
250	1·3	0·0

These results indicate that the temperature coefficient of the instrument is negligible for the range + 15° C. to + 35° C.

Effect of Variation of Wave-shape.

The effect of wave-shape upon the performance of the meter was investigated for a number of waves, some of which are reproduced as Figs. 5–10. The variations of error due to the use of these waves instead of the calibrating waves shown in Figs. 3 and 4 are given in Table 5, which includes also the Fourier analysis of the waves, up to the 11th harmonic. The amplitudes of the harmonics are given as percentages of the amplitude of the fundamental, values of less than 2 per cent being omitted.

The changes of error in Table 5 are within the limits of experimental error, and the meter may therefore be said to be independent of wave-shape, for waves normally encountered. There are, however, two types of waves which give rise to errors. One of these, which is considered in the Appendix, is not of practical importance. The other type consists of re-entrant waves; that is, waves in which the function passes through zero more than once per half-cycle. A wave of this type is illustrated in Fig. 11. In determining the mean height of this wave the small loop A should evidently be subtracted from the sum of the loops B and C, an operation which the commutator-rectifier performs correctly if the brushes are properly set. The full-wave copper-oxide rectifier, however, adds the areas of loops A, B, and C, and an error proportional to the percentage area of the loop A results. The form factor meter is therefore unsuitable for use on re-entrant waves. This fact does not greatly impair its utility, because such waves are not often encountered, and have, in any case, to be avoided in determining the losses in magnetic material; but it is a limitation which has to be borne in mind.

TABLE 5.

Wave	Harmonics, as percentages of fundamental					Form factor	Change of meter error
	3rd	5th	7th	9th	11th		
							per cent
Fig. 3	32	2	2	—	—	1·295	—
Fig. 4	28	6	—	—	—	1·055	—
Fig. 5	42	10	2	—	—	1·318	+ 0·1
Fig. 6	28	10	—	2	—	1·194	0·0
Fig. 7	40	8	4	—	—	1·145	0·0
Fig. 8	14	8	8	2	—	1·110	− 0·1
Fig. 9	16	4	4	4	14	1·151	− 0·1
Fig. 10	32	16	8	4	—	1·041	+ 0·2

Stability.

There remains the question of the stability of the copper-oxide rectifier over a period of time. No instability large enough to be detected has manifested itself during the few months that the instrument has been in use,

but this period cannot be regarded as long enough to be of much value. Fortunately, since the current consumption of the instrument is low, it is an easy matter to construct a filter network which will enable a wave substantially free from harmonics to be obtained from any normal alternator. If such a wave is applied to the meter, the pointer should lie on a red line which is provided on the scale at the reading **1·111**: in this way the accuracy can very readily be checked.

Precautions in Use.

In view of the fact that the instruments are not magnetically shielded, it is necessary to avoid placing the meter in a magnetic field alternating in synchronism with the operating voltage. It is also desirable that the supply voltage should be reasonably steady, since the sensitive dynamometer is necessarily under-damped.

Voltage Calibration.

The adjusting rheostats of the meter are marked in volts, and their reading therefore gives the r.m.s. voltage when the dynamometer indicates "Set Volts." The frequency errors of the dynamometer are of the same order as those of the complete form factor meter, and the temperature errors are also small. The accuracy of the instrument as a voltmeter is therefore mainly determined by the accuracy and openness of the rheostat scales. In the present instrument no great accuracy in the voltage scaling has been aimed at, and the errors of the voltage indication are of the order of 1 per cent.

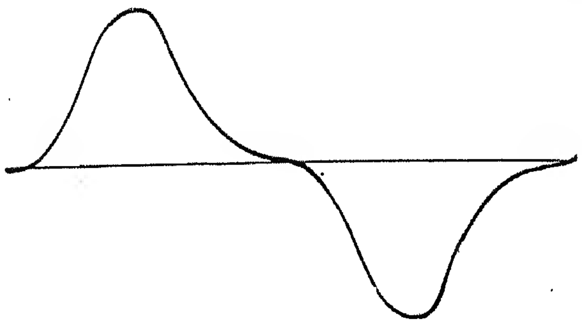


FIG. 3.

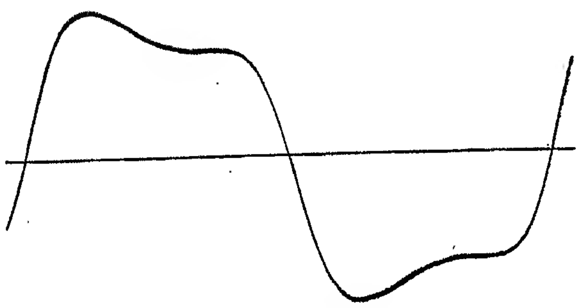


FIG. 4.

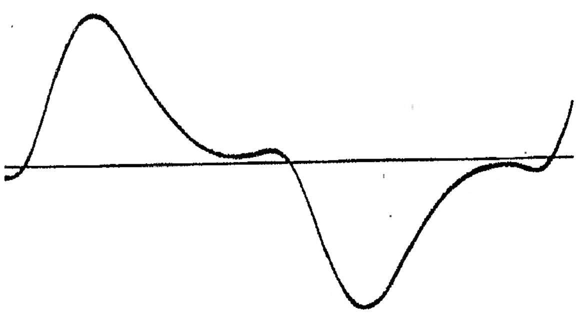


FIG. 5.

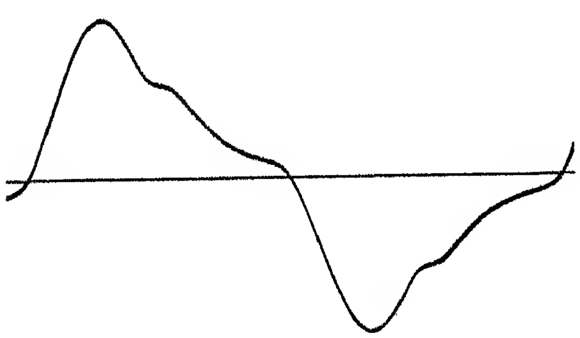


FIG. 6.

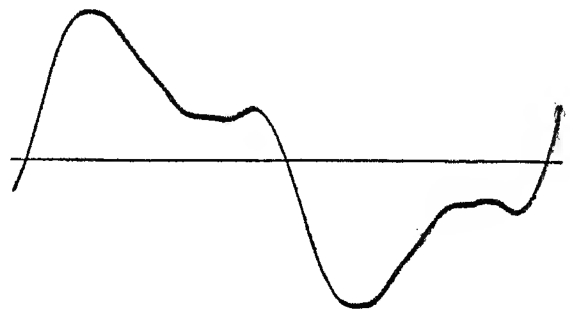


FIG. 7.

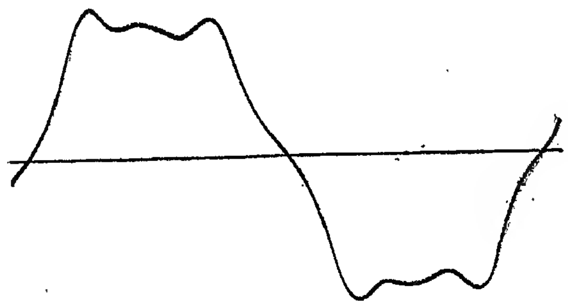


FIG. 8.

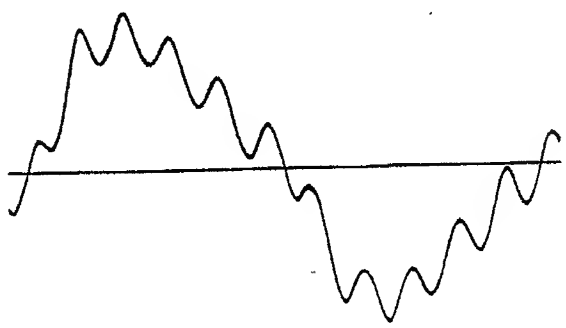


FIG. 9.

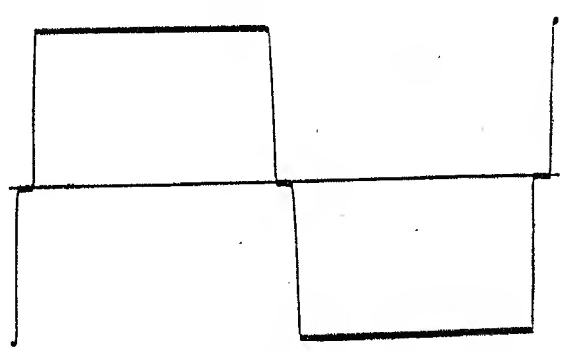


FIG. 10.

CONCLUSION.

The meter in its present form is of high accuracy, and is substantially free from errors due to normal variations of voltage, frequency, temperature, or wave-

shape. Any further development will probably aim at making the instrument suitable for a lower voltage.

In conclusion, the author desires to express his great indebtedness to his colleagues Messrs. A. Felton, C. E. Webb, and L. H. Ford, for their assistance in the determination of the performance of the meter.

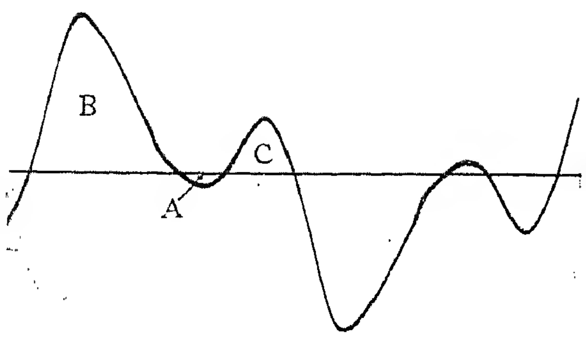


FIG. 11.

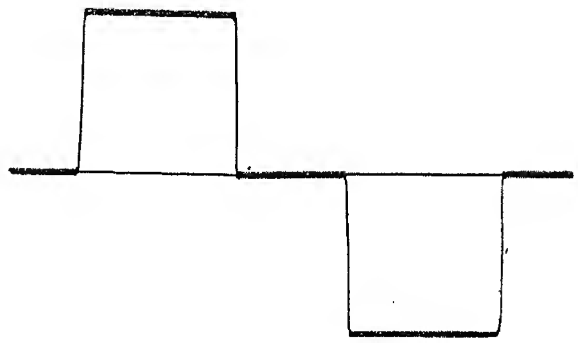


FIG. 12.

APPENDIX.

It has already been mentioned (see page 466) that non-re-entrant waves of one type cause errors in the indication of the form factor meter. The waves in question are those in which the voltage is zero for a considerable part of the half-cycle, and approximately constant for the remainder: Fig. 12 shows an example. In these circumstances the rectifier is operating for practically the whole active part of the cycle at a point on its characteristic where the efficiency is high, whereas with more normal waves the rectifier operates over the whole characteristic from zero voltage to the peak voltage, and has, on the average, a lower efficiency. In consequence, the wave shown in Fig. 12 causes the rectifier instrument to give an unduly high reading, and the indicated form factor is too low.

The calculation of the magnitude of the effect for the particular instrument under discussion is given below. The current flowing through the rectifier instrument for an applied voltage v volts can be represented in this case by

$$i \text{ (milliamperes)} = \frac{v - 0\cdot 47}{43\cdot 07}$$

from $v = 2\cdot 5$ upwards. For lower values of v the characteristic passes through the following points:—

v	i
0·2	0·0010
0·5	0·0051
1·0	0·0149
1·5	0·0253
2·0	0·0362

Consider first the wave shown in Fig. 3, and used for the determination of the errors of the higher scale-points of the meter. The values of 20 ordinates of this wave are given in the first column of Table 6, and the corresponding currents are given in the second column.

Similar calculations for other normal waves give values of the mean rectifier current, for applied voltages having a constant mean value of 18·00 volts, varying by amounts of the order of 0·1 per cent from the figure 0·4074 milliampere.

Consider now the wave shown in Fig. 12, which has a form factor of 1·320. The height of this wave, for a mean value of 18·00, is 31·36 volts, while the active part occupies a fraction of the half-cycle represented by $1/(1\cdot 320)^2$. The mean rectifier current is, therefore,

$$\frac{31\cdot 36 - 0\cdot 47}{43\cdot 07 \times (1\cdot 320)^2} = 0\cdot 4116 \text{ milliampere.}$$

TABLE 6.

Applied voltage	Rectifier current
volts	milliamperes
0·52	0·0055
1·34	0·0220
2·52	0·0476
4·00	0·0820
6·18	0·1326
9·99	0·2210
14·33	0·3218
20·31	0·4606
26·96	0·6150
36·61	0·8391
40·09	0·9199
41·11	0·9436
39·76	0·9122
36·68	0·8407
30·95	0·7077
21·63	0·4913
14·16	0·3179
8·00	0·1748
3·84	0·0782
1·02	0·0153
Mean 18·00	Mean 0·4074

The reading of the rectifier instrument will accordingly be 1·0 per cent higher for this wave than for the wave of Fig. 3, and the indicated form factor will be lower by this amount. The value of the difference actually obtained by experiment was 1·2 per cent.

The error in question is not of much importance, because it is confined to a very small class of waves. If, for example, the voltage, instead of being zero for a part of the half-cycle, has a value corresponding to 5 per cent of the maximum voltage, the indication of the meter is normal. The error is also negligible for waves of the type shown in Fig. 12, but of low form factor, as may be seen from the value given in Table 5 for the wave shown in Fig. 10.

Discussion before the Meter and Instrument Section, 2nd March, 1934, on the Papers by Dr. Hughes (see page 453) and Mr. Spilsbury (see page 463).

Mr. F. E. J. Ockenden: Dealing first with Mr. Spilsbury's paper, I gather that the great virtue of the author's form factor meter is portability, and that he regards the use of a true synchronous rectifier combined with an accurate d.c. instrument as fundamentally a more accurate method of measuring form factor than the oscillograph method. The latter, although not portable, must, I think, have advantages; because even if the uncertainty is, as he suggests, of the order of 1 per cent, nevertheless the oscillograph gives not only the form factor but also a complete view of the wave. The additional information conveyed by the latter would probably offset the odd 1 per cent of uncertainty in readings taken from the oscillograph record. As Mr. Spilsbury pointed out, his form factor meter suffers from the limitations of the rectifier itself. The wave must not be of such a shape as to operate on the rectifier at a poor point in its rectifying characteristics for a large portion of the cycle; in other words, so long as most of the wave is using the rectifier at satisfactory points on its characteristic curve the indications will be correct.

With regard to the paper by Dr. Hughes, when copper oxide rectifier-operated instruments were first introduced I considered that they could not claim a place among scientific instruments of a high order of accuracy. My colleagues, however, were more optimistic, and decided to make use of selected rectifiers. Taking all the circumstances into account I must admit that this course was fully justified since, in spite of their limitations, rectifier instruments if properly designed have features which are not possessed by any other form of indicating instrument. The author has gone into the question of their behaviour very thoroughly from an academic point of view, and it is of interest to compare his conclusions with those arrived at by practical workers in the course of some years with rectifiers. I agree with conclusion (1), page 462, which mentions that the introduction of a rectifier into a low-resistance circuit distorts the current wave and causes the rectifier ammeter to read low; but this applies only to circuits of very low resistance, where the rectifier is used merely because it gives an evenly divided instrument scale. The resistances are fairly high as a rule, however, and that characteristic is not likely to produce any great effect. Conclusion (2) must also be qualified by a statement as to the resistance of the rest of the circuit. If one is measuring microamperes in a circuit in which the potential is fairly high and the circuit resistance very high, the resistance of the rectifier is negligible by comparison, and, since the rectifier characteristics of the 1-mA type are better than those of the 10-mA type, a 1-mA rectifier would give the better result. A point which is not discussed in the paper is the question of temperature coefficients. A 10-mA rectifier used on 1/10 (i.e. 1-mA) load will show a worse temperature characteristic than a 1-mA rectifier working at full load. In general, if the current density in the rectifier is high, there will be a correspondingly large drop across the latter, but the temperature characteristics will be good. If, on the other hand, the current rating is low, the drop will be low also, but the temperature characteristic will tend to be poor; and the two factors have to be balanced one against the other. I regard conclusion (3) as self-evident, because the tendency of the rectifier is to introduce harmonics into an otherwise sinusoidal wave; and, assuming the circuit is of low resistance, an inductance will tend to choke out those harmonics, whereas a capacitance will tend to furnish a low-impedance path for them. Conclusion (4) needs a certain amount of qualification, because the d.c. resistance of a rectifier for a given current is constant and independent of the series resistance in the circuit. What the author apparently means, however, is that the effective resistance alters the wave-form of the current flowing when one is dealing with sinusoidal voltage waves and a low-resistance circuit. Conclusions (1) and (4) therefore really mean the same thing, and it is imperative to mention that conclusion (4) applies to alternating currents flowing in circuits of low resistance. It is obviously not true of a d.c. circuit. Conclusion (5) has also to be modified by the question of temperature characteristics. In its present form it means that one has to use a very low voltage in a circuit containing a rectifier instrument if the latter is to give the effective value of the current, instead of merely the mean value. In practice, however, I should not care to do this, because the rectifier would be operating under such extremely poor conditions that one could not rely upon the readings unless the instrument had been calibrated immediately beforehand at the operating temperature. Conclusion (6) states that the reading is generally much nearer the true average value than that given by an instrument worked from a rectifier of the mechanical type. I take it that this can apply only where the wave is re-entrant and cuts the zero axis twice. If not, the copper-oxide rectifier can have no advantage over the mechanical rectifier as a means of ascertaining the average value of the wave. As regards conclusion (7), the application of the rectifier to a current transformer is now so thoroughly understood that it is difficult to see how the author arrives at these views. Turning to the formula for the current-transformer performance given at the top of page 459, the author states that "M is the mutual inductance,

L_2 and R are the self-inductance and resistance respectively of the secondary circuit." His mention of the secondary circuit rather suggests that he is referring to the leakage reactance, i.e. the reactance with a closed secondary circuit; but it is apparent from the formula that he means the open-circuit inductance, and in that case it should be clearly stated that the open-circuit inductance is measured at such an induction as will be in practical use when the transformer is on load. If the transformer is working on a closed circuit having practically no impedance, the induction in the core is practically zero, and L measured at that induction would also be very small. On the other hand, if the burden on the current transformer is increased, the flux in the core must rise to supply the necessary e.m.f., and the self-inductance measured on open circuit in those conditions will be very much greater. In equation (9) the author ignores the fact that L_2 is not a fixed quantity, but is dependent on R. In other words, within the limits of the saturation of the transformer core, and the ability of the primary current to supply magnetizing current, the function $[R/(2\pi f L_2)]^2$ will be such that the correct secondary current will pass through the secondary circuit irrespective of the value of R. The ratio of a good current transformer is not greatly affected by the secondary burden. The resistance of the rectifier varies with the instantaneous value of the current, and therefore if the rectifier is connected as a secondary burden the instantaneous burden on the transformer will vary cyclically; but the flux in the core will vary cyclically in the same sense, and the ultimate tendency must be to pass the true current through the secondary circuit. The problem of obtaining the true secondary current even when the primary current is very small can be solved, as the author points out, by making L_2 large. He suggests doing this by using a large core, but the easiest way to overcome the difficulty is to make the number of secondary turns sufficiently large. Assume, for example, that the drop across the average rectifier at full load is 1 volt: if the current transformer is wound for 5 amperes the secondary burden will be 5 VA, but if a 1-mA rectifier and a transformer with 5 000 times as many secondary turns are used, the burden will be only 1 milliwatt and the inductance will be correspondingly high. I consider, therefore, that the correct solution of the current-transformer problem is not to try to use a rectifier voltmeter across a resistance, but to make the movement of the voltmeter the true secondary burden. To those who are engaged in experimental work involving the use of rectifiers the paper is of great value, since it shows clearly the nature of the errors which may arise in rectifier circuits in certain conditions. I feel, however, that if the conclusions arrived at by the author are allowed to pass without some qualification an unjustifiable sense of alarm may be caused among users of rectifier-pattern instruments.

Mr. H. Cobden Turner (remarks presented by **Mr. A. Sherwin**): For purposes where temperature changes are met with, the selenium rectifier has pronounced advantages over the copper-oxide rectifier. For instance, it has only about one-quarter of the temperature variation of copper-oxide rectifiers, and therefore compensation for temperature is more easily achieved. Dr. Drysdale recently suggested that the ideal instrument for many, if not most, purposes would be one with a logarithmic scale, and in this connection the selenium rectifier is of special use. (Mr. Sherwin here exhibited a lantern slide.) This shows a logarithmic scale produced by associating a rectifier with a moving-coil instrument which has been calibrated in decibels as well as in milliamperes. The instrument gives a close approximation to the inverse ratio of impedance with current over a wide range. The rectifier unit is not of the copper-oxide type, and the law which governs its operation varies somewhat from the 0·8 power law mentioned by the author. The instrument has various applications in the field of high-frequency measurement. (Mr. Sherwin here exhibited another lantern slide.) This shows a long-scale voltmeter, 0–100 volts. The scale departs somewhat from the linear law. This instrument has its own problems of temperature and frequency compensation, which have been dealt with in a practical manner, but the same questions of minimum current impedance and wave-form remain. The development of this instrument is due to Dr. Ryall, of the Post Office.

Mr. S. A. Stevens: I am very fortunate in having been associated with the development of the copper-oxide rectifier in this country from the time when it was first placed on the market, and even at that period (1927) the possibility of using it for measuring purposes was foreseen. One of the first instruments I ever examined fitted with this type of rectifier had been made in the laboratories of the Union Switch and Signal Co. under the direction of Dr. Grondahl, the inventor of the copper-oxide rectifier. I was very dissatisfied with the design of that instrument; it was provided with a most unsuitable rectifier, the defects of which were compensated by an extremely complicated resistance network. The first rectifier instrument which we made in this country, in 1927, was arranged as a 3-range voltmeter having scales of 1·5, 15, and 150 volts. It was designed for making measurements on railway track circuits, and it incorporated the first rectifier which was specifically designed for instrument purposes. The rectifier was of the 50-mA type. The instrument was put to practical test on a large electric signalling contract for 3 years, where it was chiefly employed as a circuit tester, being nearly always used on its 1·5-volt range. When the instrument was returned it was very dirty and the pointer was bent over at right angles, but after mechanical overhaul I could find little fault with its calibration. Since then it has had 3 years' work in a general test-room, and has often been brutally used. Its maximum errors are now about 1·5 per cent on the 150-volt range, 3 per cent on the 15-volt range, and 10 per cent on the 1·5-volt range, with the original rectifier. These figures show that the maximum errors are to be expected on the low ranges unless other steps are taken. For the highest accuracy on ranges of less than 10 volts, my advice is, do not couple the rectifier through a small resistance, so that the rectifier impedance is the main impedance in the circuit, but transform the voltage up with a small potential transformer. This need only weigh a few ounces, as the load will only be of the order of 1 milliwatt. One way of judging a rectifier instru-

ment is to look at its scale. If this is linear the instrument can be relied upon, but if the scale is contracted it means that the rectifier impedance has a controlling effect on the deflection. I made a current-transformer rectifier instrument in 1927. The core of the transformer was not made of mumetal; in fact mumetal is not necessary if the design is correct. A current transformer with about $\frac{1}{4}$ sq. in. of iron can be designed to have good frequency characteristics between 25 and 5 000 cycles per sec., because the burden on the secondary is so small. The construction of the metal rectifiers made in 1927 was on the following lines. The disc was of $\frac{3}{4}$ in. diameter, of oxidized copper, and had a lead contact washer clamped against it. Four of these elements clamped up together made a bridge rectifier. To ensure that a rectifier will give the most efficient operation on

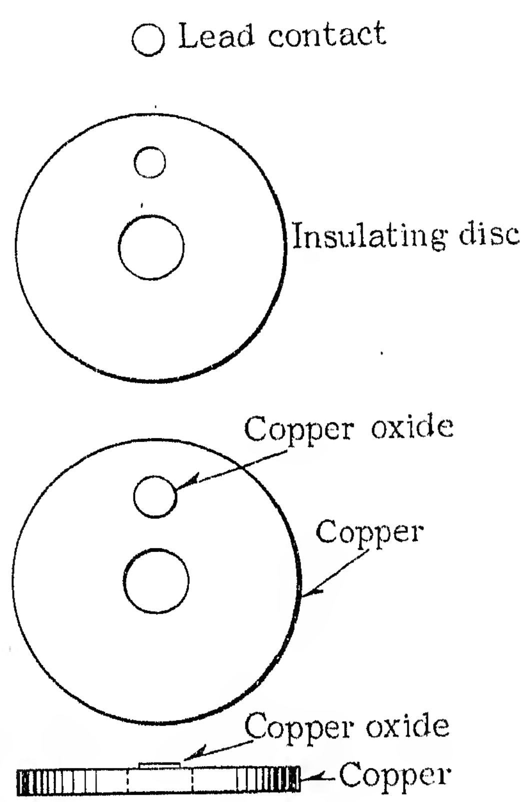
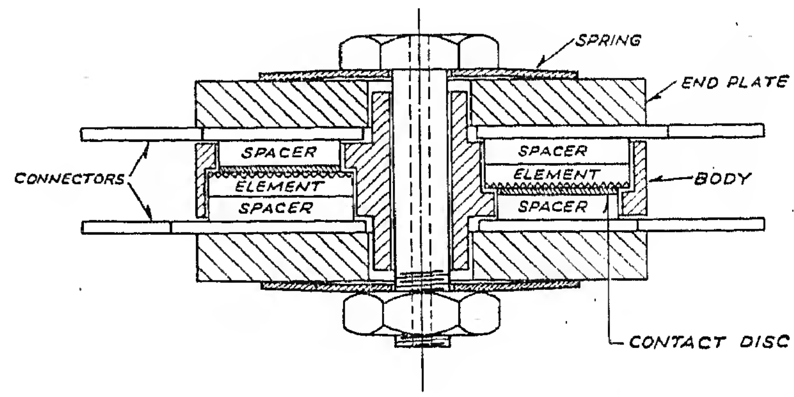


FIG. A.

an instrument, it is necessary to take into consideration the instrument load. Nearly always the instrument is required to work on a very low current consumption, of the order of 1 mA. A rectifier of $\frac{3}{4}$ in. diameter is much too large for this current, because it has a far greater reverse current than is desirable. An excessive reverse current means an excessive temperature error in the instrument. The aim of development, therefore, must be to reduce the oxide area, so as to make a small and efficient rectifier which will yet be capable of carrying the current required. We tried to oxidize tiny pieces of copper and build these up as minute rectifiers: we found that, though we could subject these small pieces of copper to the necessary treatment, the reverse current did not diminish as the area was reduced. We therefore developed, in 1929, the construction shown in Fig. A. We took the oxidized disc, etched away all but a very small portion of the oxide area, and then covered the area of bare copper left with a piece of insulating material having a hole in it, of smaller diameter than the oxide area, in which a minute lead contact was placed. This construction was a vast improvement. There is still, however, a defect in this type of rectifier, namely the frequency error, which is caused by the high self-capacitance of the adjacent pieces of copper separated only by insulating discs. The frequency error amounts to about 5 per cent at



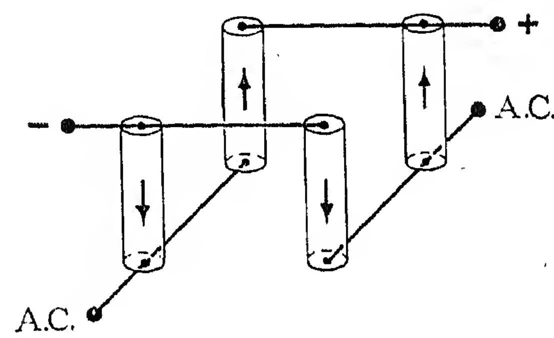


FIG. B.

10 000 cycles per sec., corresponding to a difference of less than 0·5 decibel. As a change of at least 2 decibels is necessary for the alteration to be noticed by the ear, a 5 per cent error is not of serious importance. By adopting a new method of construction (Fig. B) we have been able to produce an equally good rectifying ratio without introducing stray capacitance. This development has arisen out of the use of the metal

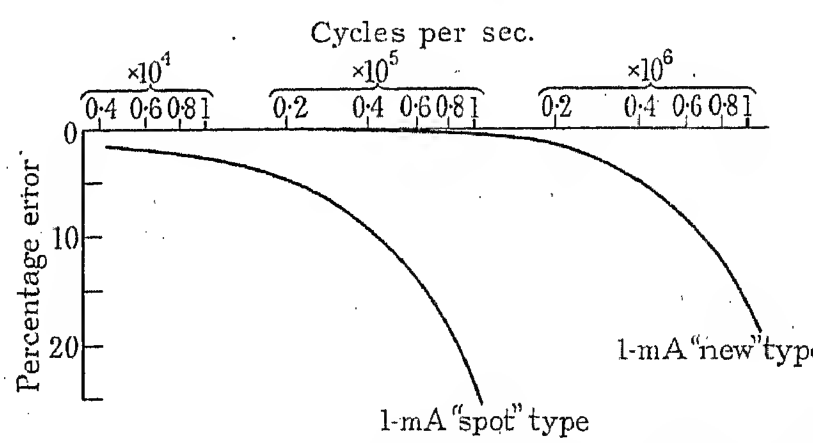


FIG. C.—Frequency error at full scale of 1-mA rectifier used in conjunction with 1-mA 100-mV moving-coil instrument.

rectifier for radio-frequency detection purposes. The main difficulty, now that we have been able to achieve a high reverse resistance with small elements, is to assemble the rectifier unit. The parts are fitted inside a complicated bakelite moulding forming the centre-piece, with bakelite clamping plates at each end backed up by spring pressure. The new type of rectifier has a frequency range of rather more than 10 times that of

the earlier type, and has a negligible error up to frequencies exceeding 100 000 cycles per sec., as shown in Fig. C. These frequency errors are obtained when reading at full scale on a 1-mA instrument with a 1-mA rectifier, the instrument movement having a resistance of 100 ohms. If the resistance of the instrument movement is higher, the voltage-drop across the rectifier will be greater, the potential to which its self-capacitance is charged will be greater, and hence there will be a bigger frequency error. The resistance and capacitance curves reproduced in Fig. D show that a bigger frequency error is obtained at a lower current density. The resistance curve follows the usual d.c. characteristic of a rectifier, and the capacitance also changes with the instantaneous value of the current passing. The capacitance curves of the old and the new types of rectifiers are of the same form, but show that we have subtracted a constant quantity of about $0 \cdot 0015\,\mu$F on the 1-mA size. There are now operating throughout the world six factories manufacturing copper-oxide rectifiers. Two of the American factories are producing instrument-type rectifiers. They have all along made

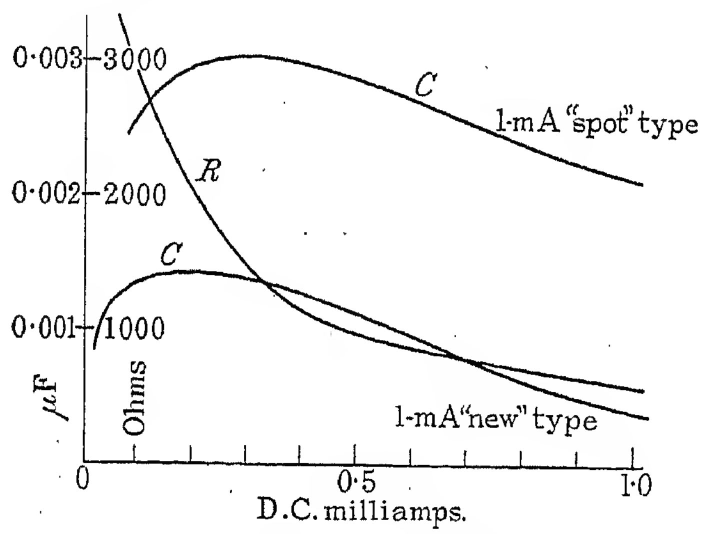


Fig. D.—Input impedance with 1-mA 100-mV moving-coil instrument.

the small-size element for this work; they have not troubled to develop it to a high rectification ratio, but have been more concerned with the frequency error. Instruments which come into this country fitted with American-made rectifiers therefore have rather bad temperature errors. The French factory had not until now made instrument rectifiers, and the great majority of the instrument-type rectifiers used in Germany have been made in this country. In fact, more instrument rectifiers have been made over here and exported than have been used in this country.

Mr. F. L. Best: The paper by Dr. Hughes makes it clear that if an a.c. voltage is offered to a rectifier the latter must distort not only the alternating current through the external portion of the circuit but also the direct current which is passed through the galvanometer receiving it. This variation of current can be regarded as due to one of two causes. In equation (1) it is expressed in such a way that there appears to be standing-back e.m.f. in the rectifier of the order of $0 \cdot 4$ volt. Alternatively, we can consider that the rectifier is similar to a galvanometer, being made of material having a large negative temperature-coefficient and zero thermal lag, so that its resistance varies from instant to instant with the current. In conclusion (4) it is stated that the effective resistance of a rectifier is lower the smaller the resistance in series. I do not quite agree with this conclusion, and indeed I am rather inclined to suspect that the reverse is the case. I cannot see how the effective resistance of a piece of apparatus can be affected by a resistance which is external to it. I can appreciate that an inductance which is not truly astatic or a condenser which is not entirely shielded can be affected by the characteristics of the external circuit, but I find it difficult to accept the fact that a rectifier can have its inherent characteristics affected by external factors. The performance of a rectifier seems to me to be determined solely by the voltage across its terminals. Turning to equations (4) and (5), let us assume that the resistance of the rectifier and galvanometer is 100 ohms. Using the author's method of determining, if the resistance of the rectifier arrangement falls, the added resistance for twice the voltage will be greater than the original impedance. For instance, if the resistance of the rectifier arrangement fell to 90 ohms, we should have to add 110 ohms to obtain 200 ohms at twice the voltage; it seems to me that it is possible for the rectifier resistance to fall, and, if it does, the results given by the author's method will be false. The effect of a swamp resistance is in fact to straighten out the deformation of the waveform due to the rectifier itself, and it is interesting to consider exactly what effect this has at the terminals of the rectifier. We know that in an iron circuit the current cannot be sinusoidal if the applied voltage is sinusoidal, because of the characteristics of iron. Similarly the current through the rectifier cannot be sinusoidal if the voltage is sinusoidal, or conversely if the current is sinusoidal then the voltage across it cannot be sinusoidal. Although the voltage across a circuit having a large amount of static resistance, as distinct from the dynamic resistance of the rectifier, may, if the swamp resistance is large, be sinusoidal, the voltage appearing across the terminals of the rectifier will not be sinusoidal, because of the internal characteristics of the latter. It is easy to make the mistake of considering that, if the oxide rectifiers were perfect as rectifiers, all d'Arsonval galvanometers calibrated on a sinusoidal wave-form in terms of r.m.s. values would read correctly on any wave-form. This is not the case. It is interesting to find that a particular value of swamp resistance will impart a square law to the voltmeter. Mr. Ockenden has questioned whether an instrument is satisfactory when it is so constructed; it seems to me, however, that by carefully arranging the circuit resistance of the voltmeter it might be possible to produce instruments which would be independent of wave-form, on account of the exponent of the voltage being 2. Mr. Stevens mentioned the idea of applying potential transformers in association with rectifiers in order to raise the voltage applied to the d'Arsonval galvanometer above the difficult value of $0 \cdot 4$ volt. Some 3 years ago I designed two millivoltmeters which offer a resistance of approximately 3 000 ohms per volt; this approaches closely the prevailing order of d.c. sensitivity. They read as low as 30 millivolts for full scale. It would appear

that an extremely low-reading millivoltmeter incorporating a potential transformer (which does not suffer from the faults we have heard of in connection with the current transformer) ought to be very useful for the measurement of switchgear contact-resistances.

With regard to Mr. Spilsbury's paper, I count myself fortunate that when the author first conceived the idea of his form factor meter he got into touch with me, and the double instrument exhibited by him is a practical outcome of that early conference. That a new form has appeared, employing a vacuo-junction, is all to the good, because it substitutes for the relatively expensive second instrument a cheap vacuo-junction plus a switch. The only drawback which I see is the one which the author has mentioned, namely that at the time of taking the form factor reading one cannot be certain that the voltage has remained constant. No doubt since the operation of changing from vacuo-junction to oxide rectifier is by means of a push-button, the galvanometer also being damped, the time-lag is not a very real danger and one can always check that the voltage remains steady. Before the vacuo-junction was incorporated I had intended to suggest that it might be an advantage to fit a reversing switch (since the dynamometer is not an astatic instrument), and one could take account of stray fields by orientating the instrument so as to be independent of them. In conclusion, I should like to mention that the freedom from error in the form factor indicator —obtained by cutting in and out practically the whole of the series resistance which controls the voltage— is due to the fact that in series with the rectifier instrument there is a fixed resistance of about 50 000 ohms.

Mr. C. L. Lipman: I think that some designers of instruments have tended to apply the copper-oxide rectifier too indiscriminately. About 1930 Mr. Stevens approached me on the question of rectifiers, and I obtained some samples from him and made up a few instrument sets, which revealed the various difficulties and errors referred to in the paper. On the one hand I wish to defend the use of rectifiers, because I think they have great future possibilities, and on the other I wish to make an attack upon them. Certain manufacturers have put on the market recording voltmeters for use on power circuits; there is no justification for using rectifiers for this purpose. They often give trouble, and in this particular application there is no need for effecting a saving in volt-ampere consumption. What is needed in such instruments is accuracy. For sensitivity in connection with measuring instruments such as milliammeters and microammeters, however, and particularly for radio work, I think there is a great field for rectifier-type instruments.

Mr. G. R. Polgreen: In Section (2) of his paper Dr. Hughes gives the impression that the value of the circuit impedance is of great importance to users of rectifier instruments. It appears from his results, however, that unless the impedance is very low the error will be comparatively small. In practice rectifier instruments are nearly always employed in those circuits where the available power is low, and in these circumstances the total circuit impedance is in general considerably higher than the values given by the curves reproduced in the paper; hence the errors will be correspondingly small. In the early rectifier instruments the large temperature error was the chief disadvantage, and elaborate external arrangements were sometimes employed to keep the instrument at an even temperature. Such difficulties led engineers to avoid using these instruments except for those purposes where no alternative existed. Recent advances have reduced temperature and frequency errors to negligible proportions, and the remaining factor affecting the accuracy is the wave-form. If the user avoids very low-impedance circuits and takes precautions to ensure that the wave is not greatly distorted from the sine form, we may truly say that these are precision instruments. Under such conditions this will apply to the numerous new applications of rectifier shunts, such as linear decibel-meters and the logarithmic-scale voltmeters and ammeters mentioned earlier in the discussion. In the large majority of practical applications these conditions hold good, since approximate sine wave-form is now the rule rather than the exception for power-frequency purposes. Supplies of sinusoidal wave-form are also employed for testing purposes in the whole audio-frequency range, for which large numbers of rectifier instruments are now being used and are found to be accurate and reliable.

Mr. W. L. Beck: I shall confine my remarks to Mr. Spilsbury's paper. In connection with iron testing, the main advantages of an instrument which reads form factor directly are that the speed of testing is greatly increased and the apparatus simplified by the elimination of the synchronous commutator and associated apparatus. Mr. Ockenden referred to the possibility of using an oscillograph for determining the form factor. In an application such as this, the time taken to resolve each wave-form into ordinates and square them in order to arrive at the form factor, is out of the question. Provided that the instrument is correctly calibrated, the errors of the single-unit instrument exhibited by the author will be of the same order as those mentioned in the paper, on any wave-form which is liable to be experienced in iron testing. The load which the instrument imposes on the secondary circuit is only of the order of 0·05 volt-ampere, and a simple correction can readily be made to allow for it in cases where it is not negligible. The author's direct-reading form factor meter makes commercial iron-testing possible with relatively simple and portable apparatus.

Mr. R. E. Swift: The paper by Dr. Hughes refers to the possible use of voltmeters for telephone work. A useful meter for this type of work is a 0–1-volt instrument of high impedance. Unfortunately, it appears from the paper that the errors due to wave-form distortion are likely to be great with instruments of this low range, owing to the low series resistance which must be incorporated to obtain the necessary sensitivity. It must not be overlooked, moreover, that when instruments of this type are employed for telephone work they may occasionally be used for indicating voltage levels on telephone circuits. The readings may fluctuate, and it may be desired to take the average value of a fluctuating reading; the more sensitive microammeter movements are of very little use for this purpose, because of their lack of damping. The low-range voltmeter presents rather a difficult problem in design, apart

from the difficulty in measuring separately the errors due to temperature, frequency, and wave-form distortion. If we compare Figs. 7 and 8 we find that with the same current and the same series resistance the 10-mA rectifier introduced the smallest error. It would be of interest, therefore, to know what errors are obtained with larger rectifiers, e.g. those rated at 50 mA. Mr. Ockenden has indicated that useful work may be done with rectifiers of this size in the low-voltage instrument range. In Section (4) the author indicates that when the exponent of the voltage remains in the vicinity of 2 over a large range, the instrument should be almost independent of wave-form. This, of course, is highly desirable, but unfortunately we find that in a 0–1-volt instrument it leads to a scale which is extremely cramped at the lower end. In telephone work a very important feature is the impedance of the instrument. It is desirable to keep the impedance as nearly non-reactive as possible, in order to facilitate the design of a suitable termination to a telephone circuit. This feature rather limits the manner in which one can correct for frequency, and it also indicates that the larger-size rectifiers may be of use here, because one can include with them a larger series resistance. Any information which the author can give of tests made on larger rectifiers would be of great value in connection with the work of designing low-range voltmeters.

Mr. D. B. Affleck: Since the impedance in series with the rectifier has a marked effect on its accuracy, owing to the distortion of the wave by the rectifier itself, when an instrument is being calibrated it should be connected in a circuit whose impedance and impedance/frequency characteristic are similar to those of the circuit in which it will be used in practice. There would appear to be considerable difficulty in calibrating a rectifier voltmeter for use at audio frequencies. Since in general the square-law relation between the current and voltage will not apply, it will be necessary to supply the rectifier with a sine-wave current. A filter will generally be inserted between the source of the current and the impedance across which the voltage is to be measured. The output impedance of the filter will affect the impedance in series with the rectifier, particularly as regards the harmonics, to which it will be reactive. As the presence of the reactance has this effect, the voltmeter will not read correctly if it is connected across a non-reactive resistance or across an impedance which differs from that across which it was calibrated. I should be glad to know whether the author can suggest any suitable means of calibrating which will reduce this error.

Dr. L. Hartshorn: There is one small point of interpretation which arises in connection with Dr. Hughes's measurement of the instrument resistance. His conclusion (4) states that the value of the effective resistance of the rectifier is lowered by adding series resistance. I am prepared to accept his conception of the effective resistance of the rectifier—it seems to me the obvious one for instrument work—but I would point out, in regard to his voltmeter-ammeter method of finding the effective resistance, that the effect of varying the resistance in series with the instrument is (as he explains) to alter its calibration factor as an ammeter. When the resistance in series with the instrument is altered, the ammeter readings are therefore made incorrect. Even if the resistance of the rectifier were constant, it would appear to vary, if the procedure outlined in the paper were adopted. A calculation which I made on noticing this showed that the error introduced was in the other direction from the effect recorded by Dr. Hughes. I therefore support the author's conclusion that adding resistance to the instrument lowers its effective resistance, but I would point out that the change is greater than the values indicated in the paper, the maximum error amounting to about 20 per cent of the actual value of the resistance.

Mr. R. Mines: Dr. Hartshorn spoke of the effective resistance of the rectifier being lowered by series resistance, and Mr. Best said he could not see how the physical resistance of the device could be affected by the value of the resistance outside it, even though this were connected in series. I agree with Mr. Best where the physical value concerned is the instantaneous resistance; but perhaps Dr. Hughes, when he speaks of the effective resistance, is referring to a value obtained by dividing the r.m.s. voltage by the r.m.s. current.

Mr. E. H. W. Banner (*communicated*): Dr. Hughes is to be congratulated on bringing to light a number of points in connection with the use of rectifiers for instruments that had previously only been suspected. The effect of the rectifier on a circuit has not, I think, been fully considered before, and it appears to me that the use of such instruments in low-power circuits, usually considered to be one of their greatest applications, is incorrect even if sine-wave calibration in r.m.s. milliamperes is performed. On the other hand, the fact that the reading on any wave-form is unlikely to be more than 10 per cent in error from the r.m.s. value is of use for defining an accuracy limit. Can it be accepted that the reading on a rectifier instrument is not the true mean value unless the circuit resistance is very high? If the rectifier is used with a voltmeter the readings appear to be r.m.s. when the range is such that the scale follows a square law, and on this basis it would appear that the best accuracy is obtainable when the range and movement resistance are such that this scale shape is obtained. Is this the author's conclusion also? For high ranges where the exponent x is unity, the calibration, if made in r.m.s. volts, is still subject to the usual form factor error. Actually, the total resistance is constant owing to the high swamp resistance, and the true mean current is read. Is it correct to assume that a shunted milliammeter which is independent of wave-form is possible if the voltage-drop is such that the scale follows a square law? Finally, it seems to me that the 1- and 5-mA rectifiers are superfluous, and that only the 10-mA unit is necessary. From this paper I have obtained useful information which will serve as a starting point for further research which I propose to undertake.

Mr. N. C. Cordingly (*communicated*): The use of rectifier instruments presents an extremely interesting problem to be investigated in the field of X-ray engineering. An accurate milliammeter is used to measure the X-ray tube current, and this, for many technical reasons, has to be fitted in the high-tension distribution system to the X-ray tube. The typical circuits employed in

X-ray high-tension equipment, together with their wave-forms, are shown in Figs. E and F. Fig. E illustrates the valve-rectifier circuits used in radiography: Fig. E(i) represents a single-valve high-tension rectifier for voltages up to 150 kV with currents of 2–200 mA, and Fig. E(ii) a 4-valve full-wave rectifier (bridge circuit) for 150 kV (peak) and 2–500 mA. Fig. F illustrates the condenser-and-valve-rectifier circuits used in X-ray treatments. Fig. F(i) represents the Villard circuit employing a valve and condensers, a voltage-doubling circuit which will supply pulsating d.c. voltages up to 200 kV (peak) and currents up to 8 mA. Fig. F(ii) represents the Greinacher circuit employing two valves and condensers, a voltage-doubling circuit which will supply a constant-potential output to the tube of 200 kV at 2–8 mA. The ideal method of control of X-ray equipment is to embody all measuring instruments and controls in a low-tension switchboard; this method is applied to the kilovoltmeter, which is calibrated in peak kV and connected to the primary side of the transformer. The milliammeter, however, in the half-wave rectifier circuit shown in Fig. E(i), is connected to the centre point of the transformer secondary, which is virtually at earth potential. The milliammeter may therefore be fitted to the control switchboard. Furthermore, since it is a moving-coil instrument it will read the true half-wave (rectified) current taken by the tube. Any charging current having alternating characteristics will not be indicated by the instrument. This method is not applicable to the other three circuits shown in Figs. E and F, since in every case alternating current is present at the centre of the secondary, thereby rendering the moving-coil instrument useless. The arrangement usually adopted, therefore, is a high-tension milliammeter in the feed to the X-ray tube. So far as the full-wave rectifier circuit shown in Fig. E(ii) is concerned, it would appear that a rectifier type of milliammeter might be connected in the secondary at its centre (earthed) point. There are, however, two or three difficulties which present themselves, and I should be glad to know whether the authors have any suggestions to put forward in this connection. The first difficulty seems to be that such an instrument would read not only the current taken by the X-ray tube, but also the charging and leakage currents of the high-tension system and transformer windings, especially if shockproof earth-sheathed cables were used. The second difficulty is that of the peculiar form factor, which in some cases may be worse than those referred to in Mr. Spilsbury's paper. As regards the Villard and Greinacher circuits, is there any possibility of using the current-transformer method? Fig. G shows a typical graph of the X-ray tube current

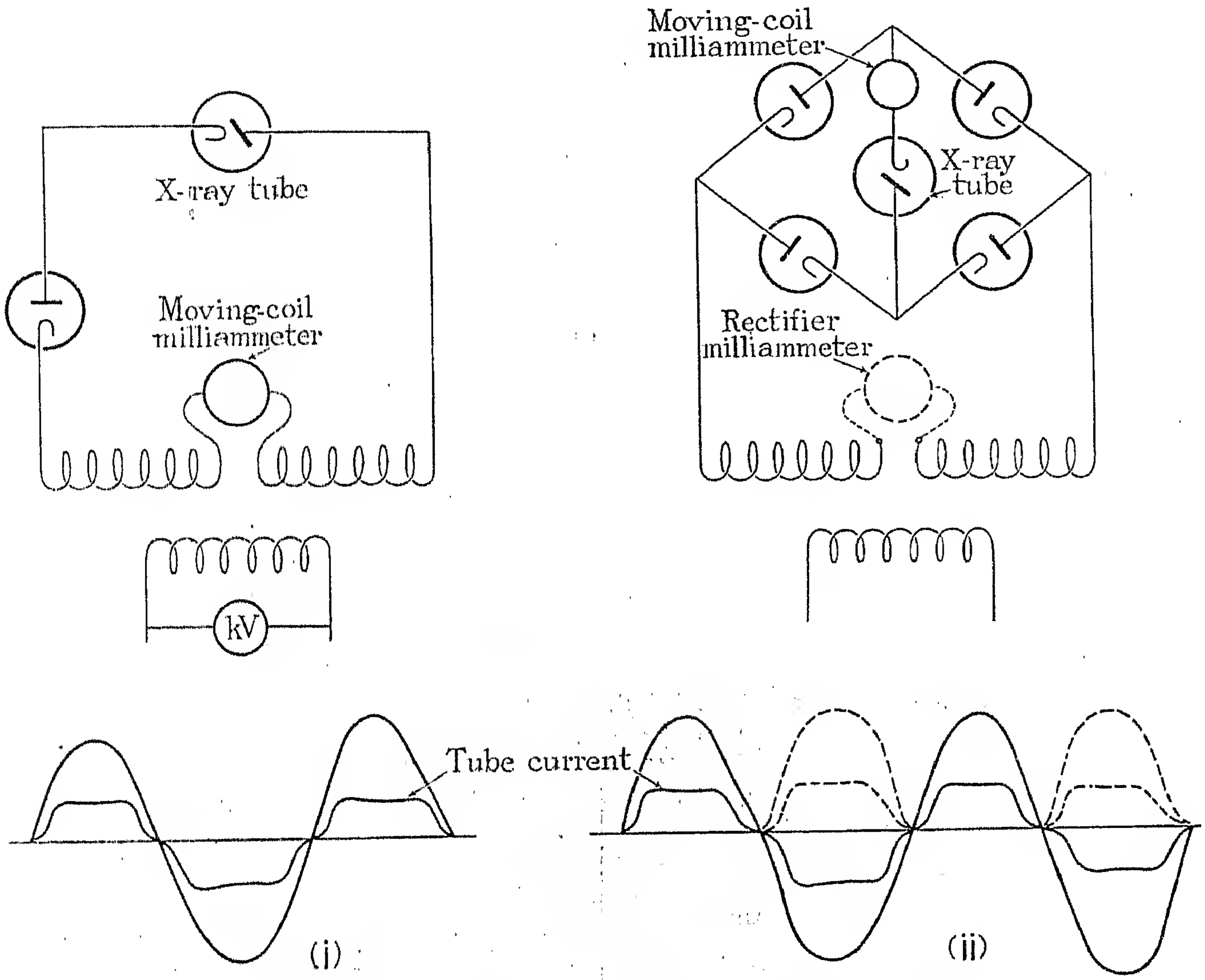


FIG. E.—Valve-rectifier circuits for radiography.

(i) 150 kV (peak), 2–200 mA load.
(ii) 150 kV (peak), 2–500 mA load.

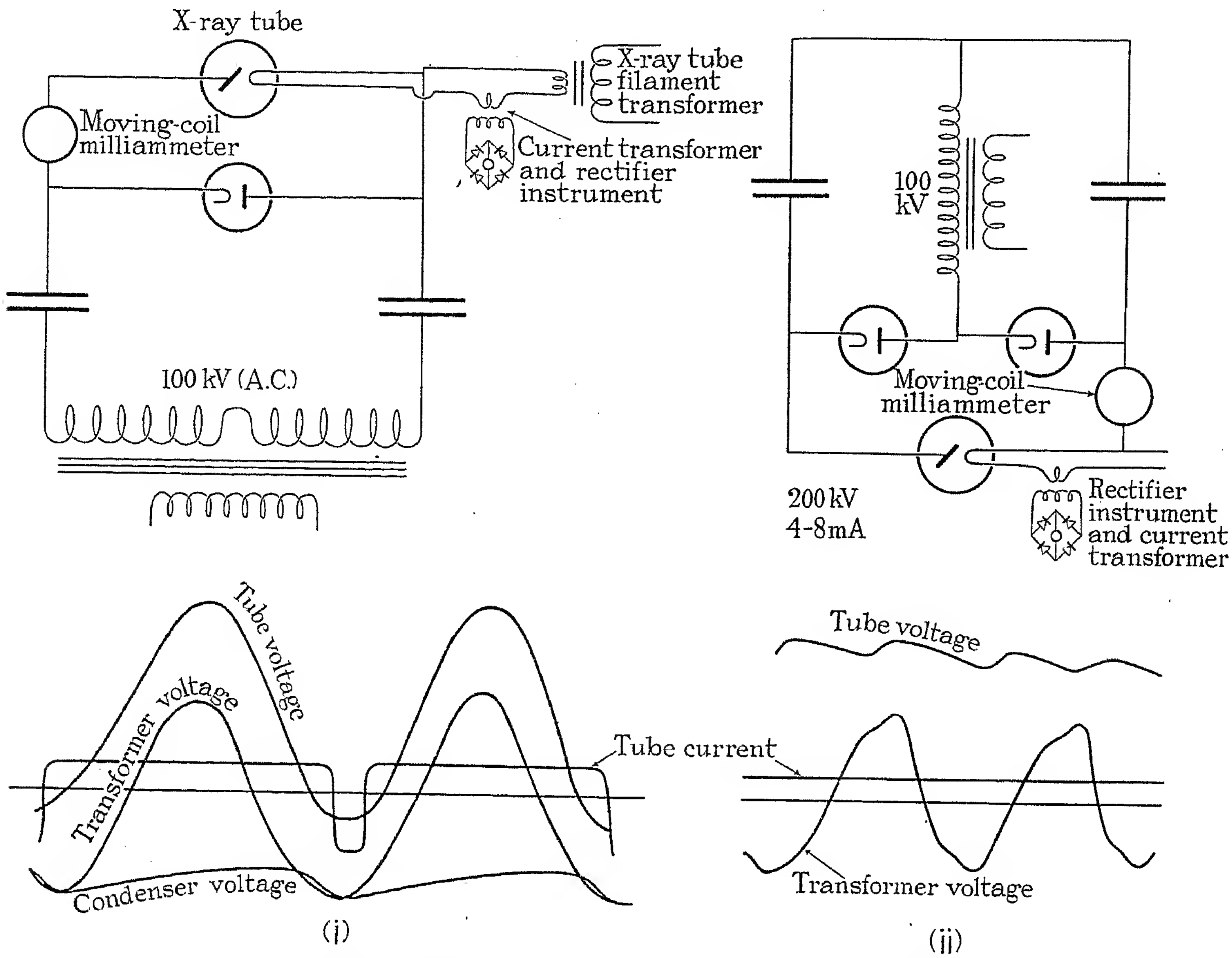


FIG. F.—Condenser-and-valve-rectifier circuits for X-ray treatments.
(i) 200 kV, 2–8 mA, Villard circuit.
(ii) 200 kV, 2–8 mA, Greinacher circuit.

plotted against the filament heating current. As the latter is of the order of 4 amperes, would it be possible,

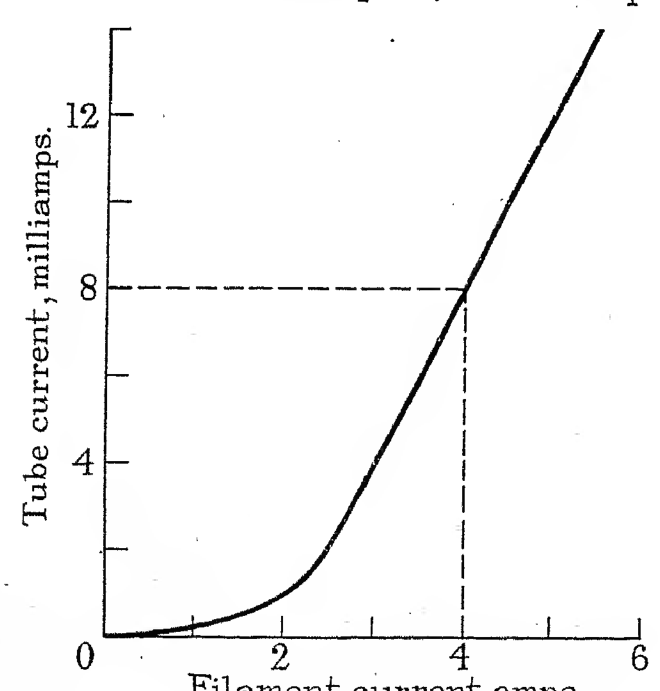


FIG. G.—Characteristics of an X-ray treatment tube for 200 kV.

by using a suitably insulated step-up current transformer, to employ a rectifier instrument which would indicate the tube milliamperes in terms of the filament heating current? Also, would the fact that this circuit is common to the high-tension circuit affect the instrument reading?

Mr. E. W. Golding (*communicated*): In connection with the determination by Dr. Hughes of the current through the rectifier when a sinusoidal voltage is applied (see Fig. 5) the graphical method illustrated in Fig. H may be of interest, since it shows fairly clearly what is happening in the rectifier circuit. The curve in the 4th quadrant represents a half-wave of the applied voltage, this being sinusoidal. The line OA in quadrant 2 represents the resistance in series with the rectifier, while added to the voltage ordinates of the line OA are the ordinates of voltage across the rectifier corresponding to the various currents through it. By projecting across from the 4th quadrant to the 2nd quadrant, and again from the 2nd and 4th quadrants to the 1st, the required current wave-form is obtained. Obviously, increase of the resistance in series with the rectifier will reduce the angle AOC and will cause the lower curved portion of the line BO to have less significance in distorting the current wave-form. The author uses a thermo-junction J in series with a resistance R_1 for the measurement of the voltage across R (Fig. 10), and states that a quadruple reading on G corresponds to

doubling the applied voltage. Since the resistance of J will vary with temperature, the accuracy of this statement appears to depend upon the relative values of the resistances R_1 and J. These values are not given. The expression $I = kV^x$ given in Section (4) means that the effective resistance of the rectifier circuit (as expressed by V/I) will be $\frac{\sqrt[x]{(I/k)}}{I}$ or $\frac{1}{k^{\frac{1}{x}} I^{1-\frac{1}{x}}}$. If the index x is 5 (corresponding to a very small value of R_2), the expression for resistance becomes $1/(k^{\frac{1}{5}} I^{0 \cdot 8})$. The index of I now agrees with the statement, made in Section (2), that the resistance of the rectifier varies approximately as $(\text{Current})^{-0 \cdot 8}$, but it seems that k must here be different from the k in the formula on page 454. In connection with the use of rectifiers with current transformers, since the readings of the rectifier ammeter give values of current which are less than the true effective value (see Figs. 2 and 3) some compensation could be obtained by reducing the turns ratio of the current transformer.

Mr. W. Phillips (*communicated*): I should like to make a few remarks on the practical application of copper-oxide rectifiers to instruments. On first reading the paper one is apt to gather the impression that copper-oxide rectifiers are liable to lead to a lot of trouble, but I would emphasize that when properly used they are exceedingly useful additions to the art of measuring small alternating currents and voltages. They are being increasingly employed in the voice-reproduction industry, where very small currents are used. As in many cases, especially for testing purposes, sinusoidal currents are employed, the rectifier milliammeter with its high sensitivity is a quite satisfactory instrument. In some cases the rectifier microammeter is being employed in preference to the telephone as a detector in a.c. bridge networks. One of the peculiarities of the copper-oxide rectifier that is impressed on the user is the change of impedance with the current. For example, consider a 5-mA rectifier having an impedance of 150 ohms with 5 mA passing: this impedance may increase to 250 ohms with $2 \cdot 5$ mA, to 500 ohms at 1 mA, and to 900 ohms at $0 \cdot 5$ mA. This change of impedance with current will greatly influence the scale shape of a low-range voltmeter when the impedance of the rectifier is an appreciable part of the total impedance of the instrument. In general, however, this will only affect voltmeters of ranges below 10 volts. Above this, scales are linear and the temperature coefficient low or practically negligible. When a copper-oxide rectifier with series resistance is employed to measure low voltages, care must be taken to choose a suitable size of rectifier and to operate this at a suitable current. In general the potential difference across the output side of the rectifier should not exceed 500 millivolts, otherwise the scale of the instrument will be contracted at the zero end, and the temperature coefficient will be appreciable and may even vary from one part of the scale to another. Rectifiers in conjunction with current transformers have been employed for some years past to measure current. The firm with which I am associated employ a special separating current transformer, usually in cascade with an ordinary switchboard current transformer, to measure current. This system is in use in the Midlands on a section of the grid, and is used to read current at a distance of some miles. The separating current transformer and rectifier are placed at the transmitting end, and the indicator consists of a sensitive d.c. milliammeter. It must be noted that when, as in this case, the current in several feeders is read by transferring the indicator, care must be taken to avoid open-circuiting the output side of the rectifier, otherwise this will be broken down and its rectifying properties destroyed.

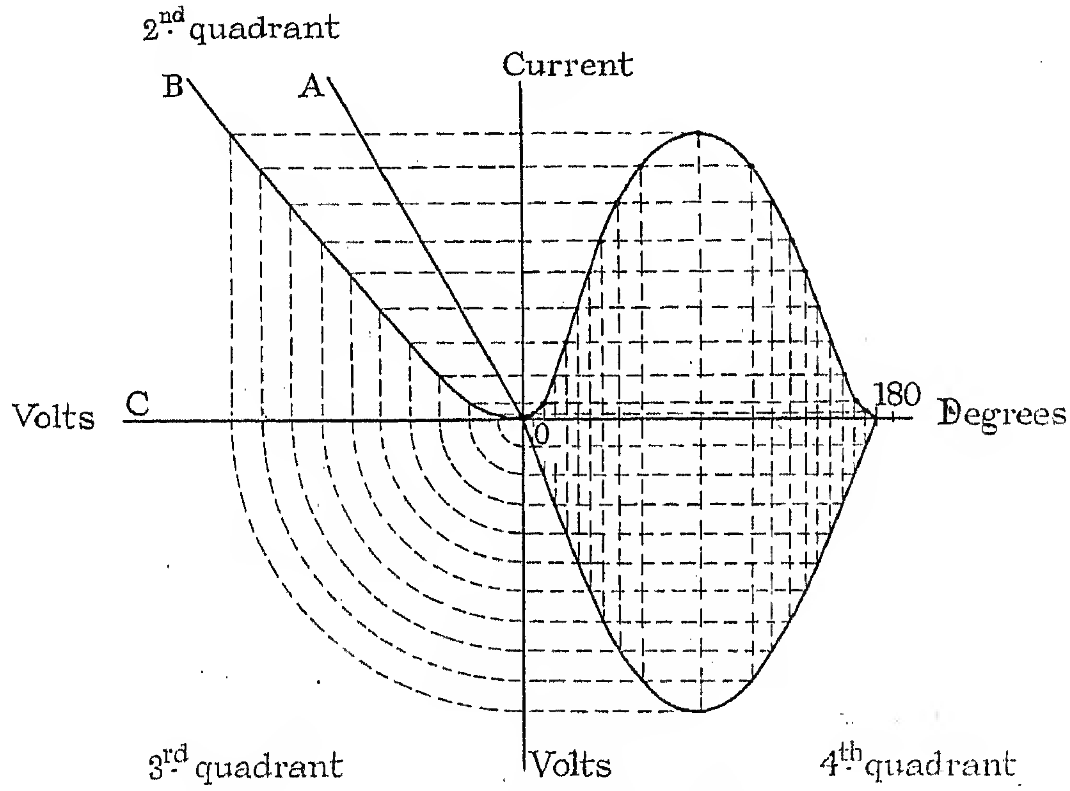


FIG. H.

The Authors' Replies to the Discussion.

Dr. E. Hughes (*in reply*):

Effect of Circuit Impedance.

Several speakers have stated that, in actual practice, error due to wave distortion is usually very small because the circuit resistance is generally high. I agree that a rectifier ammeter should only be used in circuits whose impedance is high compared with that of the rectifier. When, however, a rectifier milliammeter made and calibrated by a manufacturer of repute is available, one is naturally likely to insert it indiscriminately in any kind of circuit, partly because of the more evenly-divided scale suggested by Mr. Ockenden, but also because of its higher overload capacity compared with a thermal instrument. It is therefore essential that the limitations of the rectifier instrument be thoroughly understood, otherwise they may be discredited merely because they have been employed under unsuitable conditions.

Mr. Ockenden's qualification concerning conclusion (2) is referred to in the paper, but he has made it clearer. It should be realized, however, that a 1-mA rectifier unit, when tested with direct current, has a resistance varying from about 5 000 ohms for 100 μA up to about 30 000 ohms for 10 μA, while the corresponding values for a 10-mA unit are about 3 000 and 11 000 ohms respectively. The temperature error was not discussed, for reasons given at the beginning of the paper. At the same time, it would not be inappropriate to confirm Mr. Ockenden's remarks by pointing out that the temperature coefficient of resistance of a rectifier for the "forward" direction of the current is positive, whereas that for the "reverse" direction is negative. Further, at extremely low current densities the ratio of the "reverse" to the "forward" resistance is far smaller than at normal current densities; consequently, the ratio of rectification at very low current densities is more susceptible to temperature variation.

Mr. Ockenden suggests that conclusion (3) is self-evident. Yes! it was anticipated; but in this investigation of copper-oxide rectifiers so many unexpected effects were obtained that it was felt desirable to have the experimental confirmation given by Fig. 9.

Equivalent Resistance of a Rectifier.

The use of the term "effective resistance" in conclusion (4) has been referred to by several speakers. I believe it is made clear in the paper that this term refers only to the value observed when the rectifier is used in an a.c. circuit, and that the apparent discrepancies in the resistance are due to distortion of current caused by the rectifier. It is really obvious that when carrying a given direct current a rectifier has a certain resistance, and that this value is not affected by any resistance that is external to the rectifier. What is open to question, however, is the use of the term "effective" in the way in which it is employed in the paper. When we are dealing with an a.c. circuit possessing a resistance which varies as a function of the current, it is extremely difficult to say what is the effective resistance; even the definition suggested by Mr. Mines is not quite satisfactory. For instance, if a non-inductive resistance is connected in series with a full-wave rectifier across a sinusoidal voltage, the resistance obtained by dividing the r.m.s. value of this voltage by the r.m.s. current is not equal to the sum of the non-inductive resistance and the resistance obtained by dividing the r.m.s. voltage across the rectifier by the r.m.s. current. In other words, the resistance of a rectifier when calculated from the r.m.s. voltage and current varies for every change in the external resistance. Further, if the resistance of the rectifier is defined as the ratio of the r.m.s. voltage across to the r.m.s. current through the rectifier, the difficulty arises as to the measurement of that voltage when the rectifier is in series with a resistance, the potential difference across the rectifier being only about 1 volt.

When I was working on this question of equivalent resistance in the first instance, it seemed that the simplest and most definite procedure was to keep the mean current constant and vary the voltage and the external resistance; and I am glad to have Dr. Hartshorn's support of this conception in the case of a rectifier.

As to whether the effective resistance, interpreted in this way, should increase or decrease with increase of external resistance appears to be an exceedingly difficult matter to determine from theoretical considerations. Thus, if a sinusoidal voltage applied directly across a rectifier were to give a certain peak current, and if the voltage were then doubled and an external resistance inserted to give the same peak current, then the external resistance would equal the resistance of the rectifier when carrying that peak current and should be the same as the corresponding d.c. value. The introduction of the external resistance, however, has made the current less peaked, so that the average value of the current has increased. Consequently, in order that the mean current may remain unaltered, the external resistance has to be further increased. This, in turn, reduces the amplitude of the current and therefore increases the minimum resistance of the rectifier. In addition to these reactions there is also the variation of the rectifier resistance between a very high value and the minimum just discussed, while the current is increasing from zero to a maximum. It would therefore seem that the most satisfactory procedure is to fall back upon the experimental method described in the paper.

Effect of Series Resistance in Rectifiers used with Voltmeters.

Several speakers have referred to the possibility of utilizing the square-law characteristic of the low-range rectifier voltmeter to measure effective values of non-sinusoidal voltages. It is quite true that the error can be very considerably reduced compared with that of a linear voltmeter; but it must be remembered that, for a rectifier in series with a given resistance, the index varies for different ranges of current as shown in the Table on page 457. In replying to queries on this point I do not think I can do better than give results obtained on the 1-mA rectifier unit referred to in the above-mentioned Table. The circuit was similar to that of Fig. 10. The reactance X was made very large com-

pared with R, so that variation of the latter did not appreciably vary the magnitude or the wave-form of the current through X. The effective voltage was measured on G, and the test was performed with R_2 equal

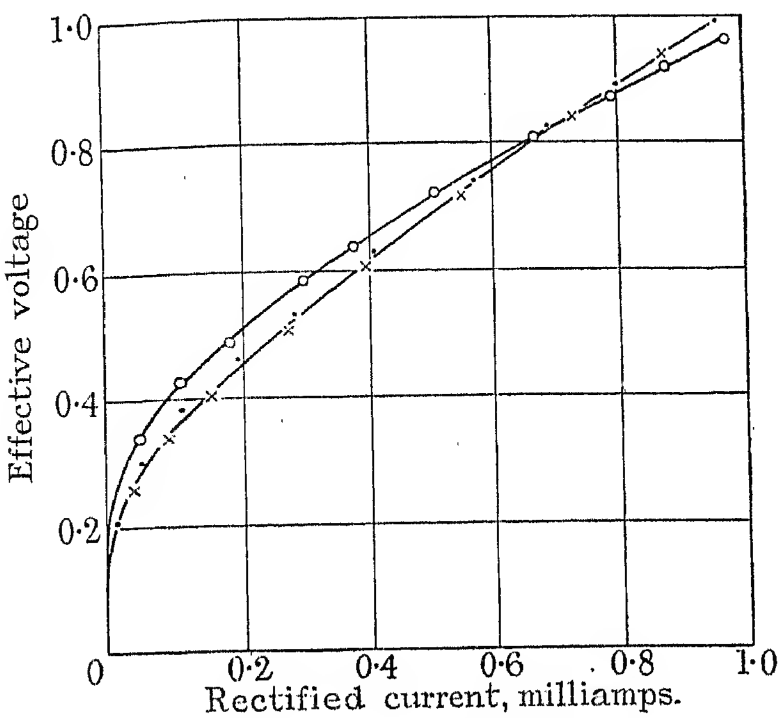


FIG. J.—60 ohms in series.

⊙ Air-core choke.
× Closed-iron-core choke.
● 22 μF.

to 60, 600, and 6 000 ohms respectively. Each of these tests was carried out with the following reactances: (i) Air-core choke to give sinusoidal current, (ii) closed-iron-core choke with 7·7 amperes having the wave-form

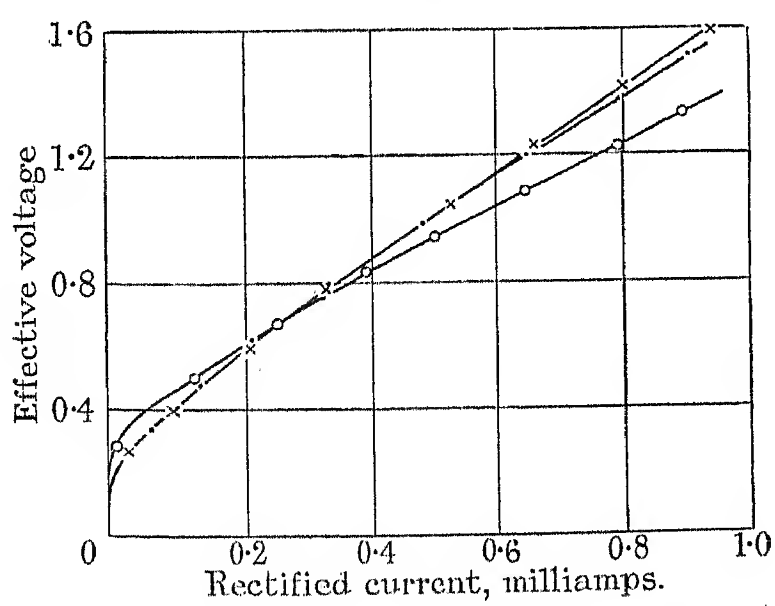


FIG. K.—600 ohms in series.

of Fig. 16(a), (iii) 22 μF taking 1·7 amperes having the wave-form of Fig. 16(c). The results are plotted in Figs. J, K, and L, and show very clearly the effects of the various indices given in the Table on page 457. It will be evident that, even on very bad wave-forms, the rectifier voltmeter can be arranged to read the effective value fairly closely. At the same time, it is necessary to emphasize Mr. Ockenden's warning concerning the temperature error under these circumstances.

With reference to Mr. Swift's query concerning larger rectifiers, I regret that I have no information available

Rectifier Instrument in Conjunction with a Current Transformer.

I am grateful to Mr. Ockenden for making it clear that L_2 in expression (9) is the open-circuit inductance of the secondary winding when the flux density is the same as that under load conditions. The numerical value given on page 460 refers to this condition. In spite of the fact that L_2 is not a constant and that the alternating quantities involved in the above expression may not be sinusoidal, the latter seemed to be the most convenient in this case. Mr. Ockenden's reference to the large core is incorrect; only a large L_2 is suggested in the paper, and with this he is in agreement.

In reply to Mr. Stevens, it is true that a mumetal

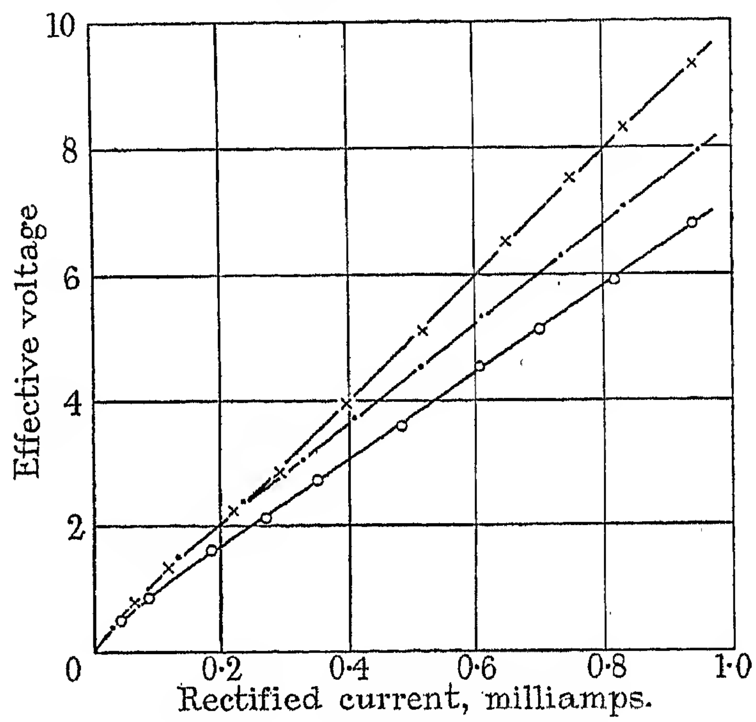


FIG. L.—6 000 ohms in series.

core is not essential. It was used because it happened to be available and enabled a high value of L_2 to be obtained without recourse to a large number of secondary turns.

General.

Mr. Turner's remarks are of great interest in showing that the selenium rectifier has now been developed to a stage where it can be used for measuring-instruments. I am grateful to Mr. Stevens for his historical survey and for dealing so fully with recent improvements of the copper-oxide rectifier.

Mr. Affleck refers to the calibration of rectifier voltmeters on audio frequencies. The voltmeters are presumably for comparatively low ranges. If they are calibrated with sinusoidal currents as described, errors must inevitably appear when they are used to measure sinusoidal voltages. With such a method of calibration, the terminal voltage of the rectifier voltmeter must be distorted. Thus, Fig. M is a cathode-ray oscillogram of the voltage across a bridge rectifier when a sinusoidal current is passing through the rectifier. An ammeter

was connected across the d.c. terminals, and for this particular oscillogram the current was about half the rating of the rectifier. It is of particular interest as showing that in the neighbourhood of zero current the voltage varies extremely rapidly, indicating that the corresponding resistance of the rectifier is very high. The same characteristic curve was obtained with rectifiers having different ratings. I would therefore suggest that a better method of calibration would be to ensure a sinusoidal terminal voltage—for example, by connecting the voltmeters across a condenser in series with an air choke adjusted to give resonance.

In reply to Mr. Banner, I would say that a rectifier voltmeter only reads the true mean value when its swamp resistance is very high, whilst a rectifier ammeter gives the mean value of the actual current wave, distorted

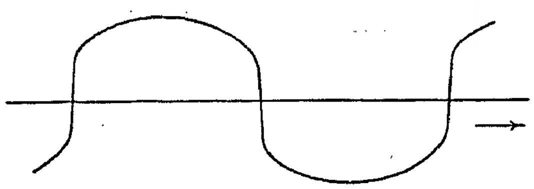


FIG. M.

though it may be by the rectifier. In both cases, however, it is assumed that the ratio of rectification is high and that the wave passes through zero only twice per cycle.

With reference to Mr. Cordingly's questions, a rectifier milliammeter used as in Fig. E(ii) would undoubtedly be useless for the purpose he has in mind, for the reasons he suggests. Also, the proposed use of a rectifier connected through a current transformer to the filament circuit does not appear to possess any advantage over the cheaper method of employing a standard current transformer in conjunction with a moving-iron ammeter.

Mr. Golding's graphical construction is very interesting and instructive. With reference to the possibility of the resistance of the vacuo-junction varying with temperature, this was checked at the time the tests were made, and the change of resistance over the full range of current was found to be negligible compared with the total resistance of the circuit. This has been confirmed by the Cambridge Instrument Co., who state that for the 50-mA vacuo-junction—the type used in my experiments—the variation of resistance between zero and rated current is approximately $+ 0\cdot3$ per cent. I am grateful to Mr. Golding for drawing attention to the agreement between the values of x in Sections (2) and (4). This was a point that I had overlooked. I also thank him for pointing out that k in Section (2) is not the same constant as k in Section (4).

Mr. R. S. J. Spilsbury (*in reply*): With reference to Mr. Ockenden's remarks, the oscillograph method of measuring form factor is, in my opinion, too laborious to be of much practical use—a view which, I observe, is shared by Mr. Beck. Further, there are few applications where an error of 1 per cent could be tolerated. Even where the actual wave-shape had to be photographed I should not make use of the oscillogram to determine the form factor. As to the choice between the synchronous-rectifier method and the use of the meter described in the paper, the former method has probably a slight advantage as regards general accuracy, and is free from error on re-entrant waves: on the other hand, the form factor meter has the advantages of cheapness and portability, and is much less liable to accidental error in the hands of moderately skilled observers. Other points which have to be taken into account in the comparison are mentioned in the paper.

I am not in agreement with Mr. Ockenden as to the cause of the error of the meter on rectangular waves of certain types, since the maximum error arises when the rectifier operates for virtually the whole active part of the cycle at a point where its efficiency is high. It will be seen from the Appendix that this error is reduced when the rectifier operates at an inefficient point for part of the cycle, and that even under these conditions the output of the rectifier is high, not low.

Mr. Best mentions a later development of the meter, in which the dynamometer is replaced by a thermo-junction, so that a single galvanometer can be used both for setting the voltage and for indicating the form factor: this modification is due to Mr. W. L. Beck of the Cambridge Instrument Co. The damping of the direct-current galvanometer is so much better than that of the low-voltage dynamometer that the constancy of the voltage can be checked as rapidly with the single as with the double instrument, while the astaticism of the arrangement is of considerable advantage. The absence of change of error of the meter with change of series resistance is due partly to the high swamp resistance in series with the rectifier, and partly to the fact that the rectifier combination is shunted by the much lower resistance of the dynamometer circuit.

I am glad to know that Mr. Beck considers that the meter will be of use for commercial iron-testing.

ELECTRODEPOSITION OF RUBBER.*

By D. F. Twiss, D.Sc.

(*Paper first received* 17*th January, and in final form* 20*th March*, 1934.)

Summary.

Rubber latex consists normally of a suspension of minute negatively charged rubber globules in an aqueous serum. Under electrolytic stress, therefore, the globules tend to migrate towards the anode.

By using anodes of suitable metal, e.g. zinc, or a porous diaphragm round the anode, it is possible to effect electrodeposition of the rubber. With suitable conditions, and using compounded latex, deposits are obtainable which can be dried and vulcanized. Commercial manufacturing processes have been based on this principle.

The possible modifications in the conditions of electrodeposition permit numerous and interesting variations in the details of the procedure and of the results.

The use of latex in this way obviates the customary need for the heavy machinery used in ordinary rubber manufacture and eliminates the preliminary milling treatment of the raw rubber, so that the mechanical properties of the product are correspondingly improved.

Introduction.

The electrodeposition of rubber makes use of the natural colloidal dispersion of rubber in water, i.e. the latex, in which form the rubber is primarily obtained from the tree. Such colloidal dispersions exhibit certain characteristic properties in general.

The microscope reveals the fact that in a colloidal dispersion, e.g. of particles of the order of 0·0001 mm diameter, the individual particles are in a state of continuous movement, vibrating and also translatory. This "Brownian" movement of such dispersions was first observed in 1827 by a botanist, R. Brown, with certain pollen grains, but he demonstrated its non-vital character by observing it also in a dispersion of a portion of finely powdered Egyptian Sphinx. The smaller the particles the more active the movement, which indeed is of the same nature as the movement of the molecules of the surrounding liquid medium.

Another feature of such suspended particles is the possession of an electric charge relative to their liquid medium. In water the dispersions more frequently carry a negative charge, but not universally so. As the majority of such dispersions are electronegative and as the rubber in Hevea latex is no exception, consideration will be devoted mainly to this type.

The electric charge has important consequences. It influences the stability of the dispersion by preventing direct collision between the moving particles; on neutralization of the charge, however, the dispersions tend to coalesce or coagulate. It also leads to migration of the particles towards the appropriate electrode if an electrical stress is set up in the dispersion; the resulting electrophoresis, a well-known phenomenon, was observed more than 40 years ago by S. E. Linder and H. Picton† with an aqueous colloidal dispersion of arsenic sulphide. Furthermore, the sign of the electric charge has a very important bearing on the nature of the coagulants effective with such dispersions. For a negative dispersion —including Hevea latex—acids are powerful coagulants, their positive hydrogen ion being readily adsorbed by the negative colloid particles with consequent neutralization and coalescence or coagulation. Similarly in soluble coagulants of saline character the positive metallic ion is the effective part of the molecule and is much more powerful the higher its valency. Soluble bivalent calcium or zinc, and particularly tervalent aluminium, salts are active coagulants for Hevea latex and other negative dispersions. It is necessary in processes of rubber manufacture using latex to incorporate various compounding ingredients, e.g. zinc oxide, and in order to prevent coagulation of the latex by any acidic or soluble metallic impurities therein previous treatment or contacting of these compounding ingredients with an alkaline solution, e.g. sodium carbonate or ammonia, is a customary precaution. In any case, the addition of negative hydroxyl ions to latex increases the stability of the latter; and indeed for the transport of latex in good condition from the East to Europe and America a small proportion of alkali (either ammonia or less frequently potassium hydroxide) is invariably introduced on the plantations.

Latex.

Although rubber can be derived from several botanical sources, all the rubber latex marketed is derived from the *Hevea brasiliensis* tree, the source of "Para rubber." As received in this country the latex is generally distinctly alkaline from the addition of ammonia (e.g. 0·5 per cent) or potassium hydroxide (e.g. 1 per cent) for stabilization purposes. The particles of rubber are commonly pear-shaped, with a major axis generally between 0·0005 mm and 0·003 mm in length; the approximate specific gravities of the rubber and serum are 0·92 and 1·02 respectively. The character of this latex as a negative colloidal dispersion was revealed in 1907 by V. Henri, who detected the Brownian movement of its rubber globules and their migration towards the anode as the result of the passage of an electric current. His results indicated the presence of about 200 millions of individual rubber globules per cm^3 of latex of 35 per cent concentration.

Some of the possibilities offered by the characteristics

* The Papers Committee invite written communications, for consideration with a view to publication, on papers published in the *Journal* without being read at a meeting. Communications (except those from abroad) should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

† *Journal of the Chemical Society*, 1892, vol. 61, p. 148.

discovered in these experiments were quickly appreciated. In 1908 a patent* was filed by T. Cockerill of Ceylon for the continuous separation of the rubber from latex (as an alternative to the ordinary coagulation methods for the production of plantation rubber) by electrodeposition on a moving anode, e.g. an endless belt covered with graphite, from which the rubber deposit was removed by a scraper. A somewhat less elaborate method for the production of plantation rubber from latex, based on a similar electrical process, was described by T. S. Clignett of Java in 1913, a shapeless coagulum being obtained which was eventually sheeted or crêped and dried. In neither case was the method developed to a commercial process.

The application of the electrical properties of latex to the direct manufacture of shaped rubber articles was delayed until 1922 or thereabouts, when the possibility of producing shaped articles directly from latex by electrodeposition was demonstrated by P. Klein† of Hungary and S. E. Sheppard and L. W. Eberlin‡ of the U.S.A.

Since that time the process has been elaborated and a considerable number of modifications and improvements together with a large number of patents for the general manufacture of rubber (and rubbered) goods from latex have been brought together under the Anode Rubber Co., and more recently under International Latex Processes, Ltd., which covers Europe and certain other parts of the world. In the U.S.A., the company known as American Anode, Inc., holds a similar control of electrodeposition processes for rubber.

Electrodeposition Process for Manufacture of Rubber Articles.

In its simplest form the principle adopted is to modify the character of the latex, e.g. by compounding, so that a stable dispersion is obtained, and to choose the working conditions so that on electrodeposition satisfactory and homogeneous shaped deposits are produced which can eventually be dried and vulcanized. The current densities applied are small, of the order of 1 ampere per square decimetre.

There are two possible methods of procedure; in one the deposit is formed directly on the anode; in the other the deposit is formed on a porous diaphragm surrounding the anode and keeping the latter from contact with the rubber dispersion.

(1) *Preparation of the Latex.*

The necessary compounding ingredients, e.g. vulcanizing agents and pigments, are introduced into the latex generally in the form of finely dispersed powders; oils also are introduced in an emulsified condition. Such dispersions and emulsions are commonly prepared with the aid of protective colloidal substances such as gum arabic, ammonium caseinate, or ammonium oleate. The presence of a small proportion of these substances effects a considerable increase in the ease of preparation and in the eventual stability of the dispersions.

In such dispersions and emulsions the dispersed particles commonly have an electric charge of similar sign to that of the latex globule. This is of importance in that otherwise their addition to latex would tend to mutual precipitation or coagulation. As will be seen later, it is perhaps of less importance for the electrodeposition process.

When an electric current is passed through latex around a suitable anode, e.g. of zinc, the anode is not insulated by the rubber deposit which forms on it. The deposit is of porous rubber and is permeated by a considerable proportion of the serum. Further deposition occurs, therefore, not on a metallic anode, but on the rubber deposit already formed on the anode (see Fig. 1). The deposition is, therefore, essentially not electrophoretic but is effected by cations, e.g. zinc ions formed at the anode and diffusing away under the electric stress towards the cathode. It is this feature which permits the building-up of rubber deposits of considerable thickness on the anode. The compounded rubber dispersion is kept well stirred, so that the electric current is not responsible for bringing the particles to the anode or anode diaphragm. The essential work of the current is to force and direct the migration of cations from the anode side of the growing deposit to its exposed surface, so that the deposit continues to grow steadily by ionic coagulation.

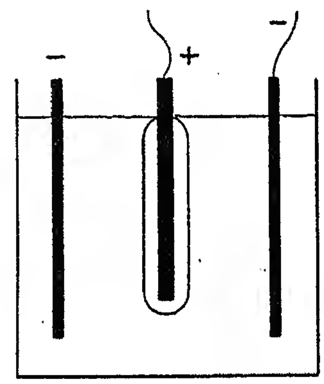


Fig. 1.

This method of formation of the deposit results in the latter having the various ingredients in the same proportion as the bath itself. The deposit is wet and may contain as much as 60 per cent of water, dependent on the experimental conditions. Under standardized conditions the proportion of water or serum will be constant, so that it is possible within limits to make use of water-soluble ingredients, e.g. accelerators of vulcanization, with a knowledge that they will be found in definite proportions in the eventually dried deposit.

By making use of the electrical endosmotic flow of water or serum which occurs through such a porous deposit cathodewards, and of factors which are known to influence this (e.g. the flow, like the rate of electrophoretic migration of the suspended particles, is greater the lower the conductivity of the bath), it is possible to raise or lower the wetness of the deposits at will. If the deposits are too dry, however, the result is an undesirable rapid increase in the electrical resistance of the deposit, so that further deposition soon flags.

(2) *Deposition on Anode Diaphragm.*

For the production of hollow rubber articles it is possible to use a suitably shaped porous diaphragm (or former), e.g. unglazed clay, around the anode in order

* British Patent No. 21441. † British Patents Nos. 223188, 223189.
‡ United States Patent No. 1476374.

to obtain a rubber deposit;* a wide range of articles such as rubber purses, pouches, and balloons, can thus be produced.

The anode diaphragm or former contains an electrolyte, e.g. calcium chloride solution, in which the anode is situated; the compounded latex is contacted with the other side of the diaphragm and makes electric connection with the cathode either directly (see Fig. 2), or indirectly by way of a second porous diaphragm enclosing an electrolyte which wets the cathode.

A particular advantage of this arrangement of deposition on an anode diaphragm is that any anodic electrolytic gases are liberated away from the deposited rubber and so cannot lead to its impairment by the inclusion of bubbles. Furthermore, there is a wide range of possible electrolytes for the anode space.

The latter feature is important because, as has already been indicated, the electric current serves by expediting and maintaining the directed movement of coagulant cations, which in this case are provided by the anodic

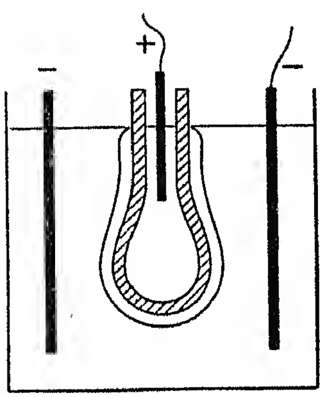


FIG. 2.

electrolyte; the nature of the deposit can be influenced by the actual type of coagulant ion effective.

The subsequent procedure is substantially the same in all modifications of the manufacture of rubber articles by electrodeposition. The deposit whilst still on its former may be dipped in a coagulant bath to increase the hardness of its exterior. It may be washed in water, the micro-reticular structure of the wet deposit permitting the removal (if desired) of soluble materials from inside the mass. It is then carefully dried. Vulcanization may be effected either during drying or subsequently. Furthermore, a latex may be used in which the rubber globules are already vulcanized, in which case a final vulcanizing operation may be unnecessary.

The article may be stripped from the former at any convenient stage after drying, before or after vulcanization, and, in some cases, even before drying.

For the production of ebonite articles or ebonite-coated articles the anode-diaphragm method is little suited. Customarily a deposit of ebonite quality (which in its simplest interpretation implies a proportion of sulphur to rubber approaching 50 per cent) is produced on a surface on which it is to remain permanently; in other words, the anode itself is an article to be ebonite-coated.

(3) *Deposition on Anode.*

Whether the deposit is to remain permanently on the anode or is to be stripped therefrom, the need to prevent concurrent liberation of gases at the anode constitutes a major additional problem. There are several methods for preventing this undesirable development, which are often used conjointly.

The proportion of hydroxyl ions in the latex (and of other anions which on discharge at an anode tend to give oxygen) should be reduced to a minimum. For this purpose much of the preservative ammonia can be removed, e.g. by evaporation, by the addition of formaldehyde (a neutral substance which, however, has the power of neutralizing ammonia), or even by dialysis.* The easy removal of the alkali is a distinct advantage of ammonia-preserved latex relative to latex preserved with mineral alkali. Direct neutralization with an acid is too delicate an operation to be attempted ordinarily, and almost inevitably causes coagulation.

Another device of similar character is to ensure that electrolytes are present such as will give no gases, whilst at the same time having a lower decomposition potential and giving if possible useful products at the anode.† Soluble sulphides in small proportion are, therefore, acceptable.

As another precaution the potential difference at the anode may be kept below that necessary for the liberation of gaseous oxygen. The most effective device is to use an anode of a metal which will be attacked at once by the liberated anions. Iron will suffice, but zinc or cadmium, particularly the former, is much more acceptable. If necessary, iron coated with zinc can be used as an alternative. The zinc not only prevents the decomposition of the discharged anions with liberation of gases, but also the products to which it gives rise (eventually zinc oxide) are desirable. The amount in any case is small.

As explained earlier, strict electrophoretic deposition occurs only initially and is succeeded by ionic coagulation by metallic cations, e.g. of zinc, migrating from the anode cathodewards. Deposition may be continued under favourable conditions until a thickness of 1 cm or so is attained.

Reduction of the proportion of electrolytes remaining in the latex bath not only decreases the tendency to gassing at the anode but also results in a greater weight of deposit per unit current. A dry weight of 3 g of deposit is obtainable per amp. per min. under good conditions, which is well over 1 000 times the rate of deposition (cathodic) of zinc; this large difference is attributable mainly to the large size of the rubber globule in latex relative to the atomic magnitude of zinc. On account of the lower total voltage in metal-plating the advantage in the weight of rubber deposited per kWh is not quite so great.

(4) *Electrical Endosmose.*

Just as the negatively charged particles in aqueous dispersion under electric stress tend to move to the anode, so a liquid in a capillary or capillaries of a solid (with a negative charge relative to the liquid) under electric stress tends to flow in the direction of the cathode.

This phenomenon of electrical endosmose has already received mention as a factor in the proportion of water

* British Patent No. 223189.

* British Patent No. 257885.

† British Patent No. 246532.

retained by the deposit of wet rubber on the anode or anode diaphragm in the electrodeposition of rubber. Electrical endosmose can play a further useful role in the electrodeposition of rubber from its aqueous dispersions.

It is important to maintain the concentration of the dispersion above a lower limit of approximately 25 per cent, because below this the rate of deposition per unit of current decreases rapidly. A uniform working concentration of 35–40 per cent is generally convenient. The deposits contain normally about 60 per cent of total solids. If the latex, compounded or otherwise, is contained in a permeable vessel, e.g. of unglazed clay, the cathode can conveniently be placed in a bath of suitable electrolyte such as dilute ammonia, surrounding the permeable latex container. When the current is passed, not only does electrodeposition take place on the anode (or anode diaphragm) but serum flows from the latex bath through the permeable wall of its container to the exterior space around the cathode; thence it can overflow to waste (see Fig. 3). Under the practical conditions

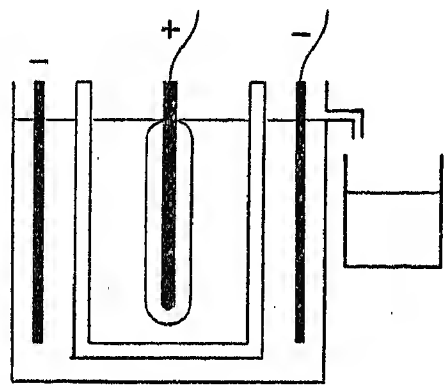


FIG. 3.

usually obtaining in the treatment of a 35–40 per cent dispersion of compounded latex, the removal of serum thus resulting from the latex bath compensates remarkably closely for the anodic loss of rubber, etc., so that the concentration of the bath is automatically maintained constant. It is necessary only to replenish the bath with additional quantities of compounded latex as fast as it becomes depleted, so that the level is maintained.

(5) *Modifications of the Simple Procedure.*

Many modifications can be introduced with respect both to the process and to the character of the products.

By using as anode a metallic drum which rotates on a horizontal axis so that only the lower portion of its curved surface makes contact with the latex at any instant, a deposit is formed which can be removed as a continuous sheet of rubber, the stripped surface of the drum returning continuously to the bath.

By a simple modification it would be possible to coat with rubber a long sheet of fabric moving at the same rate and brought against the drum at its entry into the latex.

By using a zinc mandrel (or well-galvanized iron mandrel) of appropriate size, as the anode, inner tubes for motor tyres or cycle tyres can be produced by electrodeposition.

If it is desired to form a permanent rubber coating, e.g. on door handles, handles of instruments, "dye sticks," spools for artificial silk, etc., the procedure generally will vary slightly according to the nature of the coating. If an ebonite coating is required no special modification is necessary, because the ebonite layer holds its place well and can even be produced direct on an iron or steel surface. If a soft rubber coating is to be applied it is advisable to coat the metal with a preliminary film of some special adhesive; this may be done by electrophoresis or by dipping; the deposition

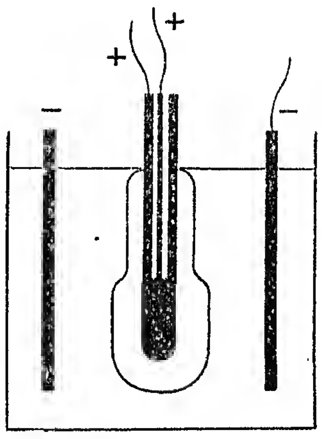


FIG. 4.

of the rubber layer is then effected, the remainder of the operations being as usual.

Another elaboration of the anode process of producing rubber articles takes advantage of the possibility of obtaining different thicknesses over different areas.* In the manufacture of articles such as a rubber boot or a bathing cap, certain areas can be separated by insulating strips so that different current densities can be used over them with corresponding local differences in the thickness of the deposit (see Fig. 4).

It is also evident that to ensure uniform deposits

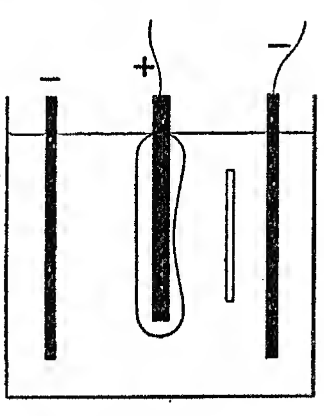


FIG. 5.

over a surface the cathode must be of such shape and size as to give uniform distribution of current over the anode surface. Conversely,† if localized differences in thickness are required, the interposition of non-conducting screens or shields of suitable size and shape between the two electrodes will lead to the desired degree of reduction in the amount deposited without causing any ugly line of demarcation (see Fig. 5).

An interesting example of the application of non-conducting areas so as to obtain arbitrarily distributed spots free from rubber is provided by the manufacture

* British Patent No. 382359.

† British Patent No. 296138.

of the Charnaux rubber corset, which consists essentially of thin sheets of high-grade soft rubber with a very complex pattern of many perforations. To make the perforations by hand-punching would require many hours of careful and monotonous work. By effecting electrodeposition of rubber on zinc sheet anodes with short rubber pegs inserted where the perforations are required (see Fig. 6), it is possible not only to ensure a perfect reproduction of the pattern each time but also to produce two corset panels at a time—one on each

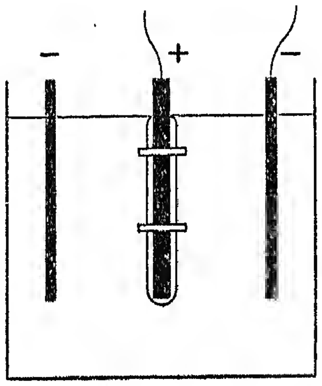


Fig. 6.

side of the anode.* The deposits are formed to the necessary thickness in a few minutes; before removal from the zinc anode they are rinsed with water and can then be dried and vulcanized.

It is possible to ring the changes in several ways on the manner of application of the electric stress necessary for electrodeposition of rubber. One interesting variation of the orthodox method assumed above is to use a carbon anode, e.g. in the form of a filled unglazed pot of the type employed in a Leclanché cell, in conjunction

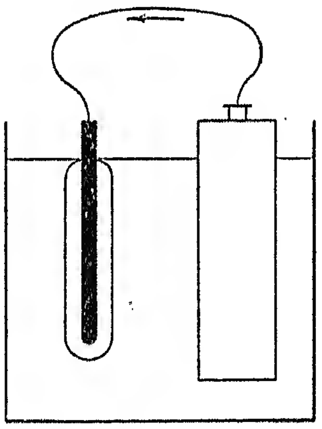


Fig. 7.

with a zinc electrode, both being immersed in latex mixture to which ammonium chloride has been added.† On electrical connection being made externally, the current flows through the latex from the zinc to the pot contents, whilst the rubber globules are deposited on the zinc electrode (see Fig. 7).

Direct current is necessary for electrodeposition of rubber, as with electroplating; but the special properties of aluminium render it possible with alternating current to use, in a bath of compounded latex, a combination of a zinc electrode and an aluminium electrode in which

* British Patent No. 358012. † British Patent No. 289965.

the latter acts as a rectifying cathode, and so obtain a normal anodic deposit of rubber on the zinc.*

(6) *Modifications in the Latex.*

Although it is advantageous for more than one reason to use preserved natural latex, artificial latex can be made and can be applied to the electrodeposition process.† By first incorporating a soluble soap or a fatty acid and an alkali (for the present purpose preferably ammonia), and then gradually introducing water, masticated rubber or reclaimed rubber can ultimately be converted into a dispersion or emulsion of rubber in water. Such artificial dispersions under present conditions are of little importance to manufacturing processes based on latex unless a softened rubber material is particularly required. The artificial latex can then be used alone, but more commonly it is added as a proportion to preserved natural latex.

Another possible modification is the inversion of the electric charge on the latex globules. This can be achieved if the latex is rendered strongly acidic or if a solution of a multivalent cation, e.g. of thorium nitrate, is added; in both cases adsorption of the positive ions introduced leads to positive charges in the globules. The inversion of the charge is best attempted after the addition of appropriate protective colloids, e.g. hæmoglobin or gelatin, which will be effective also in an acid medium, and also after substantial neutralization of the ammonia by the formaldehyde. Latex, so inverted, when submitted to electrolytic conditions shows similar behaviour to that already described, but migration is to the cathode.‡ Cathode deposition is effected preferably using a cathode capable of preventing concurrent liberation of hydrogen, e.g. a composite electrode of metallic lead and lead dioxide.

It is possible by similar treatment of a porous unglazed container with a dilute solution of thorium nitrate or even of certain organic dyestuffs, e.g. malachite green, to cause inversion of the charge on the clay relative to the water so that the direction of electrical endosmose is reversed. Cathode deposition can therefore be coupled with anodic electrosmose. Such possibilities are at present of little practical importance, no advantage being found sufficient to outweigh the inconveniences; positively charged latex, however, has shown important advantages for certain applications outside the field of electrodeposition of rubber.

In order to avoid any possibility of confusion which may reasonably arise elsewhere in descriptions of latex processes, it must be mentioned that the term "anode" process is now not always restricted, as it properly should be, to electrodeposition processes, but is sometimes applied to processes based merely on controlled coagulation (e.g. using coagulant-coated formers), i.e. to ionic coagulation without any application of electrical current.

Advantages of Electrodeposition of Rubber.

The advantages of electrodeposition are of twofold character; some are dependent on the electric side of the process; others are attributable to the use of rubber

* British Patent No. 386659. † British Patents Nos. 290107, 309030.
‡ British Patents Nos. 331563, 334581.

in an unworked condition. The main advantages arising from the electrical side have already been indicated. Some of the advantages accruing from the fact that latex permits the production of shaped rubber articles, without the customary milling of the rubber, are as follows:—

(*a*) The rubber escapes the weakening effect of the mastication process and the subsequent hot moulding, so that the final vulcanized product represents the sum of a number of positive contributions to the mechanical strength. The expensive heavy machinery of the ordinary rubber factory is eliminated, and the undesirable " grain " in calendered rubber is obviated.

(*b*) The ease of incorporation of compounding ingredients renders possible the use of a selection and preparation of accelerators of vulcanization which would inevitably lead to pre-vulcanization or " scorching " at the temperatures of the ordinary dry mixing process.

(*c*) On account of the low temperatures of vulcanization possible through (*b*), bright organic colours can be used which under ordinary vulcanizing conditions would suffer serious discoloration.

(*d*) By vulcanization of the rubber deposits in an undried condition it is possible to get a micro-porous rubber product, especially micro-porous ebonite, having marked absorbent qualities which are of use for certain purposes, e.g. for diaphragms for electric batteries and accumulators.

(*e*) The rubber is directly available for use in a fluid form without the need of mastication or of expensive and (generally) inflammable solvents, and the fluidity is greater than when such solutions are employed.

Economic Relations Between Manufacture by Electrodeposition and by Older Methods.

Electrodeposition has been proved to be a practical process for the manufacture of a very wide range of articles, including all-rubber goods and rubber- (or ebonite-) coated goods. Motor tubes, cycle tubes, pouches, rubber corsets, etc., have been manufactured in large numbers, as also have various ebonite-coated metal objects, and rubber-covered screens for sieving coke, etc. For such purposes as the last the abrasion resistance of the rubber exceeds that of a metal such as steel. The additional capacity of rubber (and ebonite) coatings for withstanding corrosion, electrical stresses, and vibration, is well known.

Electrodeposition, and indeed any manufacturing process based on the direct use of latex, is at present capable economically of producing only relatively thin rubber goods, e.g. with solid layers of rubber less than 1 cm thick. This is due in part to the difficulties entailed in the eventual drying of thicker articles. Latex sponge, e.g. for upholstery, can of course be made in large blocks, but the rubber here is not solid and drying is thereby facilitated.

Furthermore, the shipment of latex involves the transportation of a considerable proportion of water, and even if this dead weight is reduced by concentration on the plantations (e.g. by centrifugal separation of a fluid cream containing 60 per cent of rubber) the proportion of water is still considerable. In any case, therefore, the rubber in latex is at present rather more expensive than the standard forms of dry rubber, and any process of manufacture based on latex has to overcome this disadvantage. Where, therefore, a decision between a latex process and a dry process is to be based merely on cost the dry process will have the advantage; also the lower the price of raw rubber the greater proportionately will be the disadvantage accruing from the premium necessary on latex rubber. The successes of the latex process in general, therefore, are clear evidence of definite advantages in one or more of the following factors: (*a*) quality; (*b*) ease of manipulation; (*c*) economy of processing.

Seeing that progress has been so rapid in a relatively few years it would be unwise to set any limit to the range of applications for which rubber in latex form and obtained by electrochemical treatment can be given serious consideration. Ideas which appear to be beyond the bounds of possible achievement may in a short time, as they have done previously, become practical realities.*

* Most of the data with respect to processes based on the electrical characteristics of latex have appeared in the patent literature. Useful general summaries have been published by P. Klein (*Transactions of the Institution of the Rubber Industry*, 1928, vol. 4, p. 343), S. E. Sheppard and L. W. Eberlin (*Industrial and Engineering Chemistry*, 1925, vol. 17, p. 711), and C. L. Beal (*ibid.*, 1933, vol. 25, p. 609).

THE THEORETICAL AND PRACTICAL SENSITIVITIES OF GAS-FOCUSED CATHODE-RAY OSCILLOGRAPHS, AND THE EFFECTS OF THE GAS ON THEIR PERFORMANCE.*

By Prof. J. T. MacGregor-Morris, Member, and J. A. Henley, M.Sc.(Eng.), Graduate.

(*Paper first received* 1st *March, and in final form* 1st *May,* 1934.)

Summary.

A brief survey is given of recent modifications in the construction of gas-focused cathode-ray oscillographs, and some of the resulting improvements in performance are described.

The authors then summarize recent work of other investigators which has a direct bearing on the subject matter of the present paper.

A comparison is made of the electrostatic sensitivities obtained from simple electron dynamics and that found by experiment for two focusing gases, argon and helium; an explanation is advanced for the results obtained.

The phenomenon of origin distortion is next considered, and results are given showing its variation with gas pressure for hydrogen, and with the frequency of the deflecting voltage between 0 and $1 \cdot 25 \times 10^6$ cycles per sec. for hydrogen, helium, and argon, as focusing gases.

The dependence of gas focusing on the transverse speed of the electron beam is discussed, and photographs are given which demonstrate this dependence in the cases of hydrogen, helium, and argon.

The paper concludes by indicating the practical importance of the results given.

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(5) Gas focusing.
(6) Conclusion.
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(1) Introduction.

One of the most noteworthy developments in electrical measurements during recent years has been the range of measurements which have been made possible by the use of the gas-focused sealed-off type of cathode-ray oscillograph. Excellent tubes can now be obtained, both in this country and abroad, at a moderate price and with a uniformity of performance which is approaching that of mass-produced thermionic valves.

A better understanding both of the action of the Wehnelt cylinder and of gas focusing, has resulted in lower permissible gas pressures and more efficient production of the electron beam, with consequent improvement in the brightness of the fluorescent spot and diminution in spurious illumination of the screen and in the tube.

In a paper by Dobke† a description is given of the recently introduced A.E.G. type of indirectly heated cathode, by use of which the life of the tube has been greatly increased; this is due to the fact that the emitting surface is now almost completely shielded from the ionic bombardment which formerly caused its deterioration.

Tubes made by Messrs. A. C. Cossor have been improved by the introduction of an anode extension to enclose the deflecting plates, thus largely shielding them from the return space-current and stray charges on the glass walls; this has resulted in an improvement in the linearity of response and in a decreased deflector-plate conductance.

Fluorescent screens have been greatly improved, and from the range of materials now available they can be constructed to suit many uses with different degrees of visual and actinic brilliance, and with after-glow of long or short duration. These improvements have been largely due to the work of Sir Herbert Jackson and the staff of the British Scientific Instruments Research Association.

A recent report of the Department of Scientific and Industrial Research‡ should be consulted for a very complete and useful description of the modern oscillograph and its applications.

Although, in comparison with mechanical oscillographs, the moving part of a cathode-ray oscillograph is virtually inertialess, yet in connection with some measurements to which gas-focused oscillographs have been applied it cannot be assumed that a calibration made at one frequency will hold for another; or that the performance of the tube will remain unaltered over a wide range of deflecting frequencies; it is also incorrect to assume that the electrostatic sensitivity of the tube will be that given by simple theory.

The present paper is an account of a study of the connection between theoretical and practical electrostatic sensitivities and also the extent to which the performance of gas-focused tubes varies with deflecting frequency.

The tubes used in this research employed the normal Cossor electrode structure, so that the results given are directly applicable to similar oscillographs. The helium-filled tube used was in every way a standard type, helium being the usual focusing gas.

* This paper is to be read and discussed at a meeting of the Meter and Instrument Section during the session 1934–1935. The principal parts of the paper formed a dissertation approved for the Degree of Master of Science (Engineering) in the University of London, by J. A. Henley.

† See Bibliography, (1).

‡ *Ibid.*, (2).

(2) Summary of Recent Work of other Investigators.

No results have yet been published to show the variation of origin distortion with gas pressure, and, before the commencement of this research, no work had been published on the variation of sensitivity with frequency for gas-focused tubes. Since then, Hollmann* has studied the effect of very high frequencies on sensitivity due to the finite time of passage of the electrons between the deflector plates; this effect, however, only becomes appreciable at about 10^8 cycles per sec. Heimann† has given results for the sensitivity variation of tubes using the A.E.G. electrode structure and with hydrogen, argon, and xenon, as focusing gases at 0–10^6 cycles per sec.

The variation of gas focusing with deflecting frequency has hitherto received little attention, although von Ardenne‡ has dealt briefly with the effect, and the report§ referred to above contains a photograph illustrating variation of focus with transverse beam speed.

(3) Comparison of Theoretical and Experimental Sensitivities.

Johnson,‖ when using a Western Electric tube, observed that the sensitivity which he obtained by experiment was considerably greater than that calculated by simple theory; he found that the difference was equivalent to a decrease in the accelerating voltage of about 50 volts. In a paper with which one of the present authors was associated,¶ results were given for a Standard Telephones 4018A tube; and a practical sensitivity some 40 per cent greater than the theoretical was obtained.

Since there is such a marked discrepancy between the accepted theory and actual measurement it is evident that there is some factor influencing sensitivity which is not accounted for in the simple theory. Some modifications are required if this theory is to be applied to the oscillograph in its present form; alternatively, it may be possible to remove the cause of this discrepancy, after it has been discovered, by altering the design of the oscillograph.

From the usual rough theory of electron deflection,

$$\text{Sensitivity} = l_1 l_2/(2dV) \text{ cm per volt} \quad . \quad . \quad (1)$$

where l_1 = axial length of deflecting plates (cm),
l_2 = distance from centre of deflecting plates to screen (cm),
d = distance between deflecting plates (cm),
V = accelerating potential difference (volts).

The theoretical sensitivity is found by applying this expression to the electrode system of the oscillograph.

With the Cossor electrode structure (shown in Fig. 1), measurements of the distance between the deflecting plates after the assembly of the oscillograph are rendered difficult by the presence of the anode extension, which encloses the deflecting plates. It was considered unsuitable, however, to make measurements on the plates before mounting in the glass envelope, because of possible subsequent movement. It was therefore decided to apply two different optical methods to the complete oscillographs.

* See Bibliography, (5). † *Ibid.*, (6). ‡ *Ibid.*, (7). § *Ibid.*, (2). ‖ *Ibid.*, (3). ¶ *Ibid.*, (4).

Using an oscillograph with argon as the focusing gas, an X-ray photograph (Fig. 11, Plate 1) was taken, and measured by Mr. Holliday, of East London College, with a travelling microscope. The distance between the plates given by this method is certainly accurate to within 1 per cent.

A second oscillograph, containing helium, was measured directly by using a cathetometer focused on the deflecting plates through the fluorescent screen, employing direct sunlight as the illuminant; this gave the required distance to within about 2 per cent.

In calculating the sensitivity, the electrostatic field

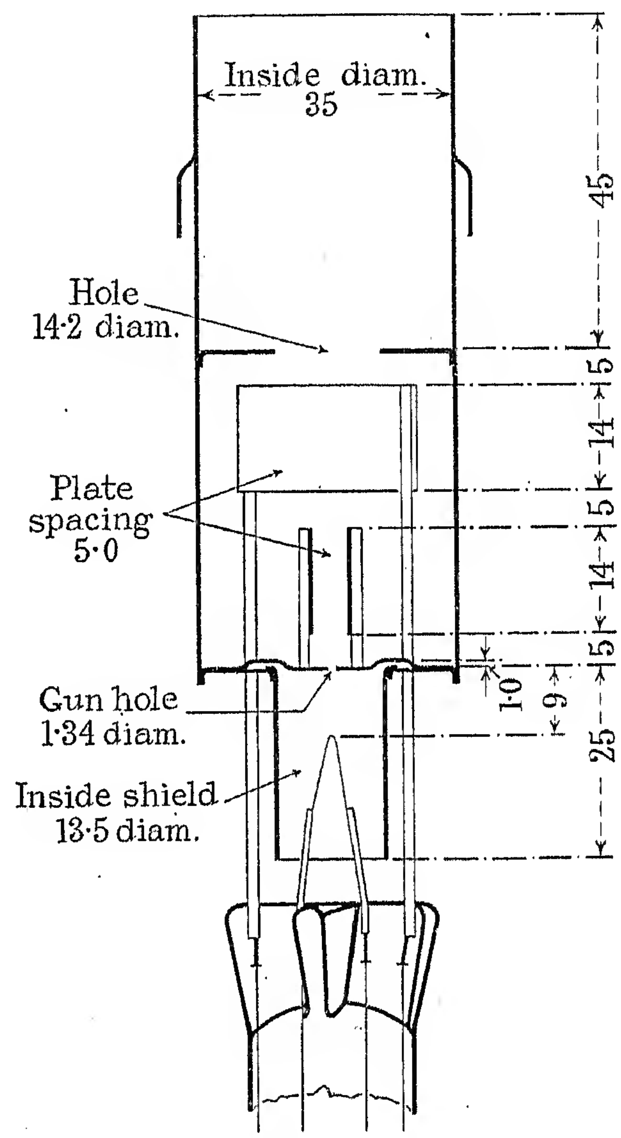

Fig. 1.—Schematic section of the Cossor cathode-ray tube. (All dimensions in millimetres.)

between the deflecting plates was assumed to be uniform, but an effective plate length was used which was greater than the actual length in order to take into account the fringing of the field. A sufficiently close approximation to the effective plate length was obtained by using a well-known formula for the capacitance of a parallel-plate condenser; the effective plate length was thus found to be 1·38 cm in place of the actual dimension of 1·30 cm, i.e. an increase of about 6 per cent.

To obtain the practical sensitivity, deflections were measured with d.c. deflecting voltages for several anode voltages, and, by using the slope of each of the resulting voltage-deflection lines as the sensitivity for a particular anode voltage, errors due to origin distortion are eliminated.

This difference can be allowed for most simply in practice by crediting the tube with an effective anode voltage which will give the actual sensitivity when used in equation (1). This effective voltage is given in Tables 3 and 4.

With both tubes, the ratio of the theoretical and practical sensitivities is nearly constant, and therefore the effective anode voltage will be nearly proportional to the true anode voltage.

TABLE 1.

Argon-focused oscillograph; pressure 8×10^{-4} mm of mercury; d measured from an X-ray photograph; $l_1 = 1 \cdot 38$ cm, $d = 0 \cdot 496$ cm; sensitivity from (1) = $326/V$ mm per volt.

Anode voltage	Theoretical sensitivity	Practical sensitivity	Ratio of Practical to Theoretical
volts	mm per volt	mm per volt	
300	1·09	1·47	1·35
500	0·654	0·84	1·285
700	0·467	0·6	1·285
900	0·363	0·47	1·295

TABLE 2.

Helium-focused oscillograph; pressure $9 \cdot 6 \times 10^{-3}$ mm of mercury; d (measured by cathetometer) = $0 \cdot 485$ cm; $l_1 = 1 \cdot 38$ cm; sensitivity from (1) = $335/V$ mm per volt.

Anode voltage	Theoretical sensitivity	Practical sensitivity	Ratio of Practical to Theoretical
volts	mm per volt	mm per volt	
300	1·15	1·65	1·435
500	0·67	0·94	1·40
700	0·48	0·68	1·42
900	0·37	0·51	1·37

It is shown below that the abnormal practical sensitivity is due to a reduction in electron velocity in the tube and that this reduction occurs largely after the electrons have passed the deflecting plates. The use of an effective anode voltage, however (as in Tables 3 and 4), assumes that the whole velocity reduction takes place before the electrons reach the deflecting plates; thus though this method is purely empirical it has the advantage of simplicity in application.

In calculating the theoretical sensitivity it was assumed that: (*a*) The electrons are initially accelerated by the full anode voltage. (*b*) The force exerted by the deflecting field is as given by simple electrostatic theory. (*c*) No force acts on the electron after it has left the anode. Since the beam passes through a small hole in the anode, and since effects due to gas are small, it may be assumed that all electrons fall through the full anode voltage. Also, from the linearity of response after origin distortion has disappeared, it is evident that the theoretical deflecting field is acting on the beam; since the only possible cause of an abnormal sensitivity is the focusing gas, which would also cause non-linearity. It is therefore apparent that the increased sensitivity must be due to the action of a retarding force which acts on the beam after it has emerged from the anode hole.

When one deflecting plate of a pair is connected to the anode, and a deflecting voltage is applied to the other, the mean potential of the plates with respect to the anode will alter, and so the velocity of the electrons will change between the anode and the plates according to

TABLE 3.

Argon-focused tube; pressure 8×10^{-4} mm of mercury; average effective anode voltage = $0 \cdot 77 \times$ actual anode voltage; average actual sensitivity = $326/(0 \cdot 77 V)$.

Anode voltage	Effective anode voltage	Difference	Ratio of Effective to Actual
volts	volts	volts	
300	222	78	0·742
500	389	111	0·778
700	544	156	0·778
900	695	205	0·772

TABLE 4.

Helium-focused tube; pressure $9 \cdot 6 \times 10^{-3}$ mm of mercury; average effective anode voltage = $0 \cdot 71 \times$ actual anode voltage; average actual sensitivity = $335/(0 \cdot 71 V)$.

Anode voltage	Effective anode voltage	Difference	Ratio of Effective to Actual
volts	volts	volts	
300	209	91	0·697
500	357	143	0·714
700	493	207	0·705
900	657	303	0·730

the value of the deflecting voltage; this additional accelerating voltage being approximately half the deflecting voltage.* This effect was negligible for the range of deflecting voltages used; thus the effect which causes the retardation must be due to a negative potential gradient encountered by the electrons between the deflecting field and the fluorescent screen.

The authors consider that the presence of the retarding field is a consequence of the mechanism by which the beam electrons return to the anode. It seems certain that, as has been suggested by Bedford,† the beam electrons return to the anode largely by secondary emission from the fluorescent screen, and not, as had previously been supposed, entirely by interchange of electrons in the ionized gas. This latter theory is untenable with modern low-pressure tubes, since there are not sufficient ions to neutralize the whole charge.

The fluorescent spot may be regarded as the cathode

* See Bibliography, (2), p. 16.
† Discussion on paper by J. T. MacGregor-Morris and H. Wright (see Bibliography, 4).

of a diode, the emission being excited by the impact of the primary electrons; the secondary current between this virtual cathode and the anode will depend upon the difference of potential between them according to the usual diode theory, although there will be slight modifications due to the presence of the gas. This secondary current must be equal to the beam current for equilibrium, so that the potential difference between the fluorescent spot and anode will depend upon the beam current.

It is thus probable that the fluorescent screen will take a potential below that of the anode, the potential difference between them depending upon the beam current and the emitting characteristics of the fluorescent screen. This potential difference gives rise to the field retarding the primary electrons by acting upon them between the anode and the screen. In Cossor tubes this field is largely removed from the deflecting plates by the anode extension; it has been pointed out by Bedford* that this is shown by the reduction in deflecting-plate current which has resulted from using the extension, thus indicating that 90 per cent of the return space current is collected by the extension. With tubes which do not employ an anode extension the effect due to the reduction in velocity before the beam reaches the deflecting plates will also be small, since they are near the anode.

The principal effect will, then, be due to a retarding of the beam electrons after they have left the deflecting plates. A calculation follows showing the magnitude of the screen potential necessary to cause the effects observed in Tables 1 and 2.

Let V' = potential of fluorescent spot above anode potential (electrostatic units), and let t = time taken by an electron to travel from the deflecting plates to the screen (sec.). Since it would be difficult to make an accurate analysis of the retarding field acting on the beam electrons it has been assumed here that the field extends from the fluorescent screen to the anode and is uniform along the beam. Because of this, the results obtained must be regarded as an interesting analysis of the problem rather than an accurate estimate of the screen potential.

Acceleration along axis due to $V' = \dfrac{e}{m}\dfrac{V'}{l_2}$

Initial velocity of electrons along axis $= \sqrt{\left(2\dfrac{e}{m}V\right)}$

Final velocity of electrons along axis $= \sqrt{\left[2\dfrac{e}{m}(V+V')\right]}$

$$\sqrt{2\left[\frac{e}{m}(V+V')\right]} = \sqrt{\left(2\frac{e}{m}V\right)} + \frac{e}{m}\frac{V'}{l_2}t$$

$$t = \left\{\sqrt{\left[2\frac{e}{m}(V+V')\right]} - \sqrt{\left(2\frac{e}{m}V\right)}\right\} \Big/ \left(\frac{e}{m}\frac{V'}{l_2}\right)$$

Velocity of electrons perpendicular to axis

$$= \frac{E}{d}\frac{e}{m}\frac{l_1}{\sqrt{\left(2\frac{e}{m}V\right)}}$$

* *Loc. cit.*

Deflection

$$= \frac{E}{d}\frac{e}{m}\frac{l_1}{\sqrt{\left(2\frac{e}{m}V\right)}} \frac{\sqrt{\left[2\frac{e}{m}(V+V')\right]} - \sqrt{\left[2\frac{e}{m}V\right]}}{\left(\frac{e}{m}\frac{V'}{l_2}\right)}$$

$$= \frac{El_1l_2}{d}\frac{\sqrt{(V+V')} - \sqrt{V}}{V'\sqrt{V}}$$

If y = sensitivity, in cm per electrostatic unit (actual);
$y' = l_1l_2/(2Vd)$ = simple theoretical sensitivity;
$a = y/y'$ = (actual sensitivity) / (theoretical sensitivity);

then $$y = 2Vy'\frac{\sqrt{(V+V')} - \sqrt{V}}{V'\sqrt{V}}$$

and, on solving for V', we have

$$V' = 4V(1-a)/a^2$$

In Tables 5 and 6, values of V' are given which are calculated from the values of y given in the third columns of Tables 1 and 2 respectively.

TABLE 5.

Argon-focused tube; pressure 8×10^{-4} mm of mercury; screen potential to give practical sensitivity as in Table 1.

Anode voltage	Screen potential, volts above anode
volts	
300	−235
500	−347
700	−488
900	−618

TABLE 6.

Helium-focused tube; pressure $9{\cdot}6 \times 10^{-3}$ mm of mercury; screen potential to give practical sensitivity as in Table 2.

Anode voltage	Screen potential, volts above anode
volts	
300	−263
500	−413
700	−588
900	−718

The reason for the difference between the two tubes is not clear, but it is probably due rather to difference between the properties of the screens than to the nature of the focusing gas. It is hoped that, in a further investigation, it will be possible to determine the difference between the practical and theoretical sensitivities for several similar tubes; and also to make a direct measurement of the screen potential.

An interesting effect which can readily be explained by the above theory is illustrated in Fig. 12 (Plate 1, facing page 496). As the anode voltage is decreased the secondary emission falls off, and, since the gas ions are not sufficient to neutralize the negative charge on the screen,

it will increase until at some threshold value the beam is entirely repelled from the screen. The tube used to obtain Fig. 12 had a rather poor screen with uneven emission; when a low anode voltage and a large beam current are used a local charged area forms on the screen as shown, and the beam spreads round it. This serves to illustrate one effect of a screen charge; such local charges do not form on modern types of screens, which function satisfactorily until at about 150 volts the fluorescent spot suddenly disappears from the screen completely.

(4) Origin Distortion.

The phenomenon of origin distortion in gas-focused cathode-ray oscillographs has been studied by many investigators.* In the present paper it is proposed to treat with two aspects of it only, namely its dependence upon the nature of the focusing gas under varying conditions of (*a*) the gas pressure and (*b*) the periodic time of the e.m.f. which is being recorded.

(*a*) *Dependence upon Pressure of Focusing Gas.*

In the path of the electron beam mid-way between the deflecting plates, positive ions and electrons will be formed by collision at a rate depending upon the efficiency of ionization of the gas by the electrons, and on the beam current. When a field is set up between the deflecting plates, the secondary electrons will tend to move towards the positive plate and the positive ions towards the negative; the time of passage of either from the beam to the plate will depend upon their mass and the strength of the field between the plates.

Since the mass of an electron is small compared with that of a positive ion, and since positive ions and electrons are produced at an equal rate in the path of the beam, there will be at any instant a greater number of positive ions than electrons remaining between the plates, and these will form a space charge which will diminish the resultant field between the plates—and hence the deflection of the beam.

As the deflecting-plate voltage is increased the time of passage of an ion to the negative plate decreases, until the space charge between the plates is negligible, when origin distortion will disappear. Origin distortion will thus depend upon the rate of formation of ions in the path of the beam, which will be proportional to the gas pressure, and also upon the molecular weight of the focusing gas. The variation of origin distortion with gas pressure has been studied for hydrogen, although it gives a smaller origin distortion than other gases for a given gas pressure, because its pressure can more easily be altered by absorption by charcoal than is the case with the heavier inert gases.

The oscillograph used was of the normal Cossor type with the exception that two side tubes were attached, one containing absorbent charcoal and the other fitted with a triode ionization gauge by means of which the pressure in the tube could be measured by the ion current indicated by a microammeter. The pressure in the tube was varied over a suitable range by altering the temperature of the charcoal. The ionization gauge was subsequently calibrated against a McLeod gauge in the Research Department of Messrs. A. C. Cossor, and the pressures were found to be correct to within 15 per cent. Two pairs of deflecting plates were used, and supplied at 50 cycles per sec. The shift along the axis to give each of the parallel lines shown in the plates was obtained by using a direct voltage, variable in steps, in series with one deflecting plate and the a.c. supply.

The results obtained are shown in Figs. 13, 14, 15, and 16 (Plate 1). In Fig. 13 the tube was very hard, and although origin distortion is not noticeable the focusing action of the gas was negligible. It was found that the trace tended to wander slightly on the screen, probably owing to stray charges on the glass. Fig. 15 was taken at about the optimum focusing pressure, while in Fig. 16 the pressure is rather high for it to be possible to focus without varying the filament emission. Another plate was taken at $8 \cdot 5 \times 10^{-3}$ mm of mercury, but the contrast between the trace and the rest of the screen was so poor (because of secondary illumination) that the plate was too fogged to be of use.

It would thus appear that in tubes using gas focusing the presence of origin distortion is unavoidable with simple electrostatic deflection, since it is caused by the gas ions to which the focusing action is due, unless special devices are used.

(*b*) *Variation with Electrostatic Deflecting Frequency.*

The resultant field between the deflecting plates for small deflecting voltages depends upon the rate of formation of positive ions between the plates and the time of passage of an ion from the path of the beam to the negative plate. If the deflecting voltage varies sufficiently rapidly with time there will be more positive ions present between the plates for a given instantaneous voltage than there would be with a steady voltage of the same value: in other words, the origin distortion will increase.

If t sec. is the time of passage of a positive ion from the beam to the negative deflecting plate, and E is the deflecting voltage,

For helium,

$$e/m = 7 \cdot 2 \times 10^{13} \text{ electrostatic units per gramme.}$$
$$t = (1 \cdot 02/\sqrt{E}) \times 10^{-6} \text{ sec.}$$

For argon,

$$e/m = 0 \cdot 72 \times 10^{13} \text{ electrostatic units per gramme.}$$
$$t = (10 \cdot 2/\sqrt{E}) \times 10^{-6} \text{ sec.}$$

For the sake of simplicity these times are calculated for the field strength assuming no ionic space charge; actually the field will be less and t greater; but the values of t given indicate the order of deflecting frequencies at which a variation in origin distortion will become apparent.

It can thus be seen that gas-focused cathode-ray tubes will become less sensitive with increasing deflecting frequency because of the inertia of the gas ions; the variation will vary also with the molecular weight of the focusing gas.

In order to obtain voltage/deflection characteristics

* See Bibliography, (2), (3), (4), and (9).

at various frequencies, the connections of Fig. 2 were used. A simple single-valve oscillator was coupled to two plates of the oscillograph, the frequency of the oscillator being variable over a wide range by means of

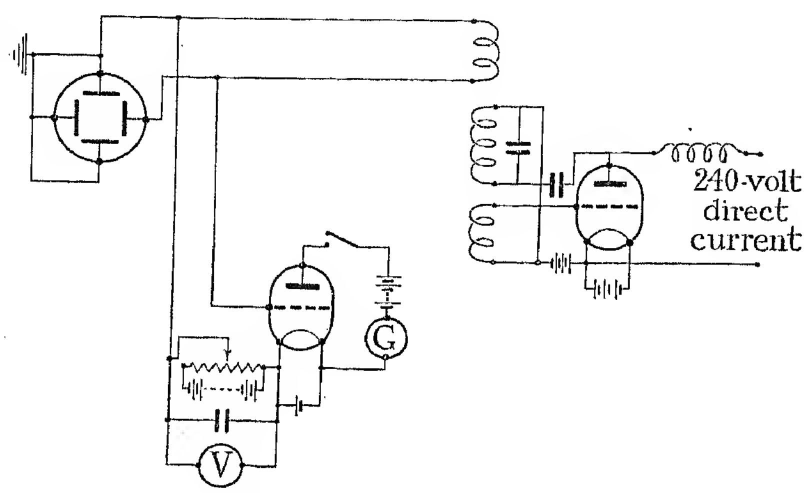

FIG. 2.—Connections for sensitivity measurements.

plug-in coils. The peak deflecting voltage was measured by means of a "slide-back" valve voltmeter connected across the deflecting plates. Frequency error of the

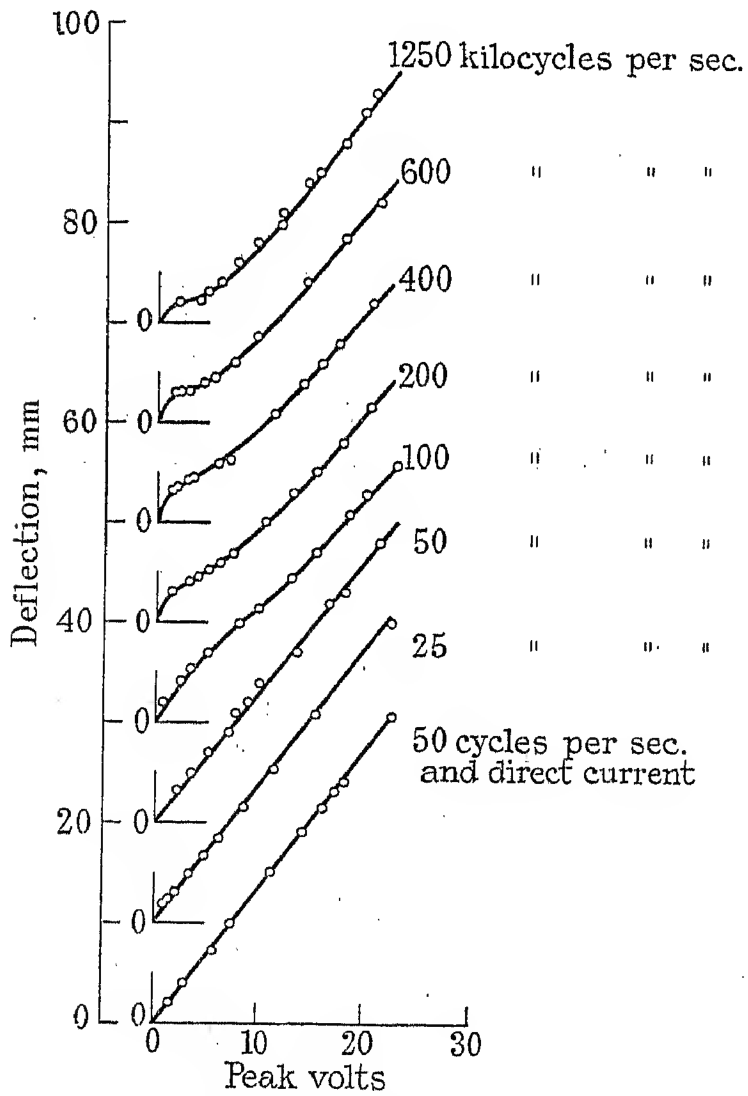

FIG. 3.—Helium tube.
$E_a = 300$ V. $I_a = 40\ \mu$A.

voltmeter was avoided by shunting the grid circuit with a suitable condenser.

The peak voltmeter was found to give readings of the order of accuracy of the readings of deflection, which were made by means of a piece of millimetre graph

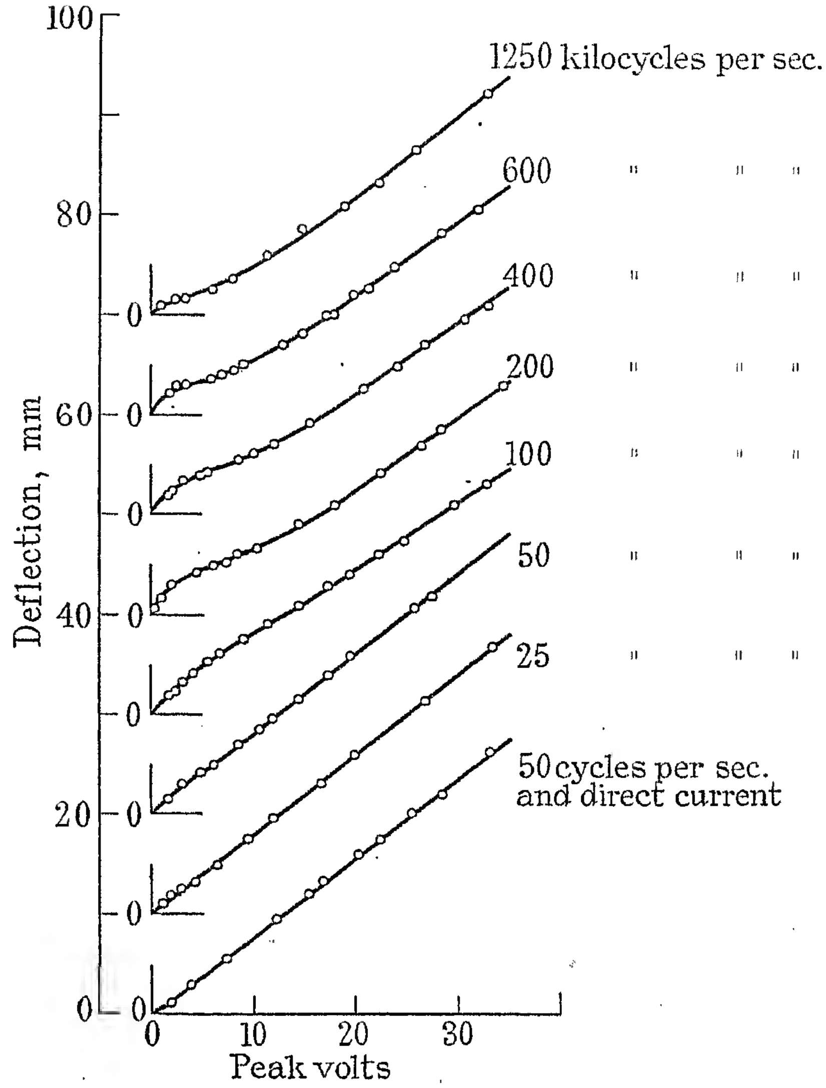

FIG. 4.—Helium tube.
$E_a = 500$ V. $I_a = 63\ \mu$A.

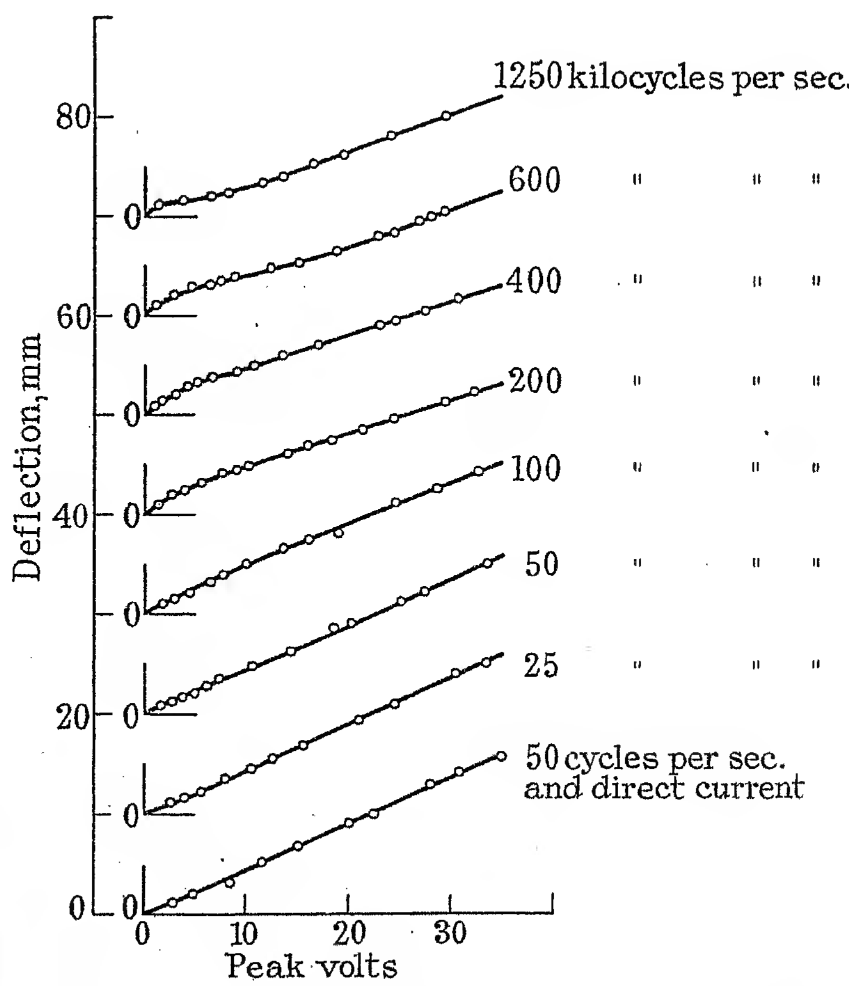

FIG. 5.—Helium tube.
$E_a = 900$ V. $I_a = 160\ \mu$A.

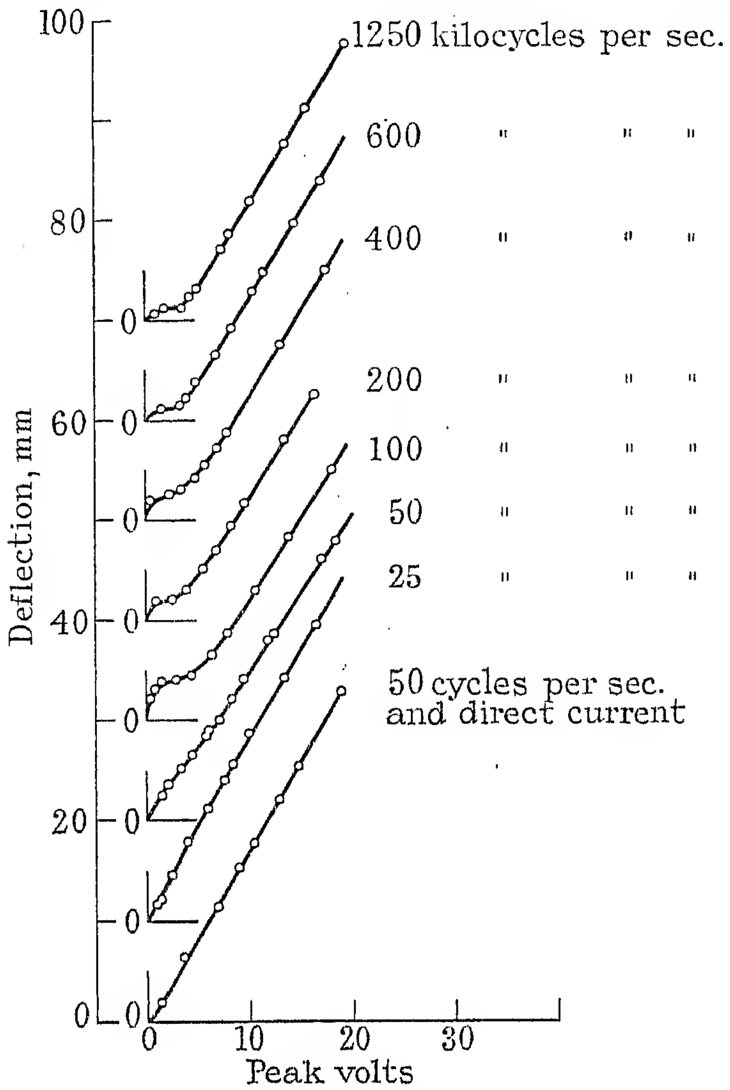

FIG. 6.—Argon tube.
$E_a = 300$ V. $I_a = 25\ \mu$A.

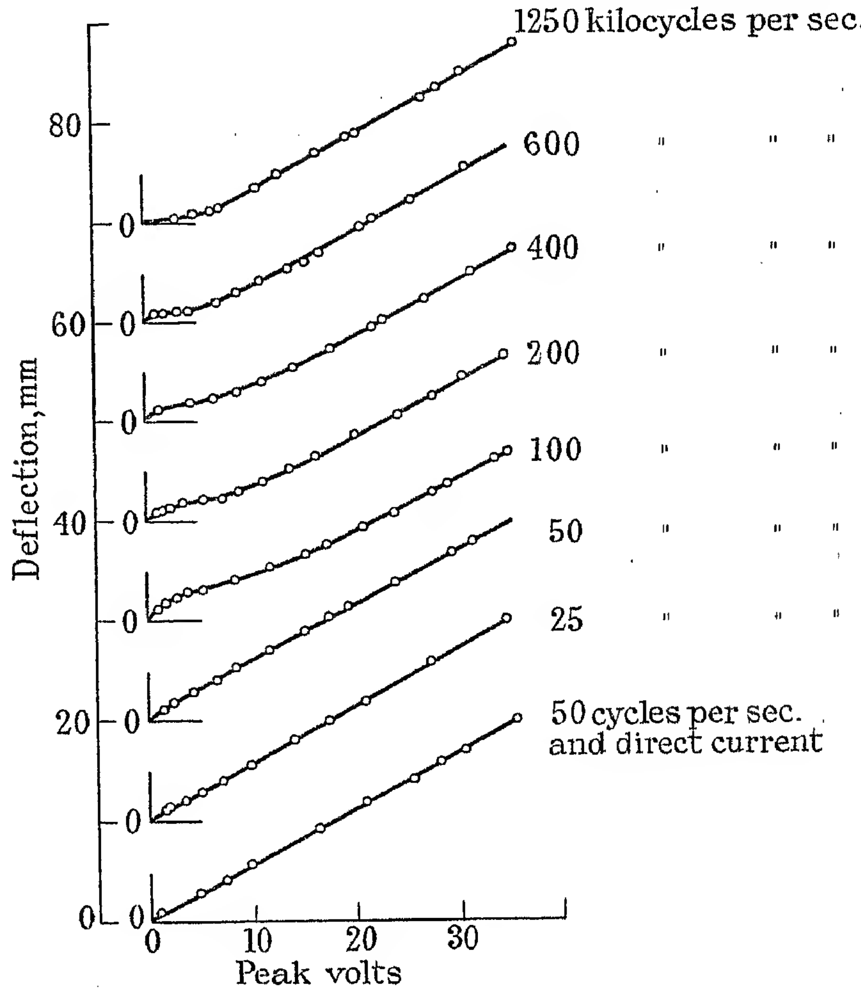

FIG. 8.—Argon tube.
$E_a = 900$ V. $I_a = 100\ \mu$A.

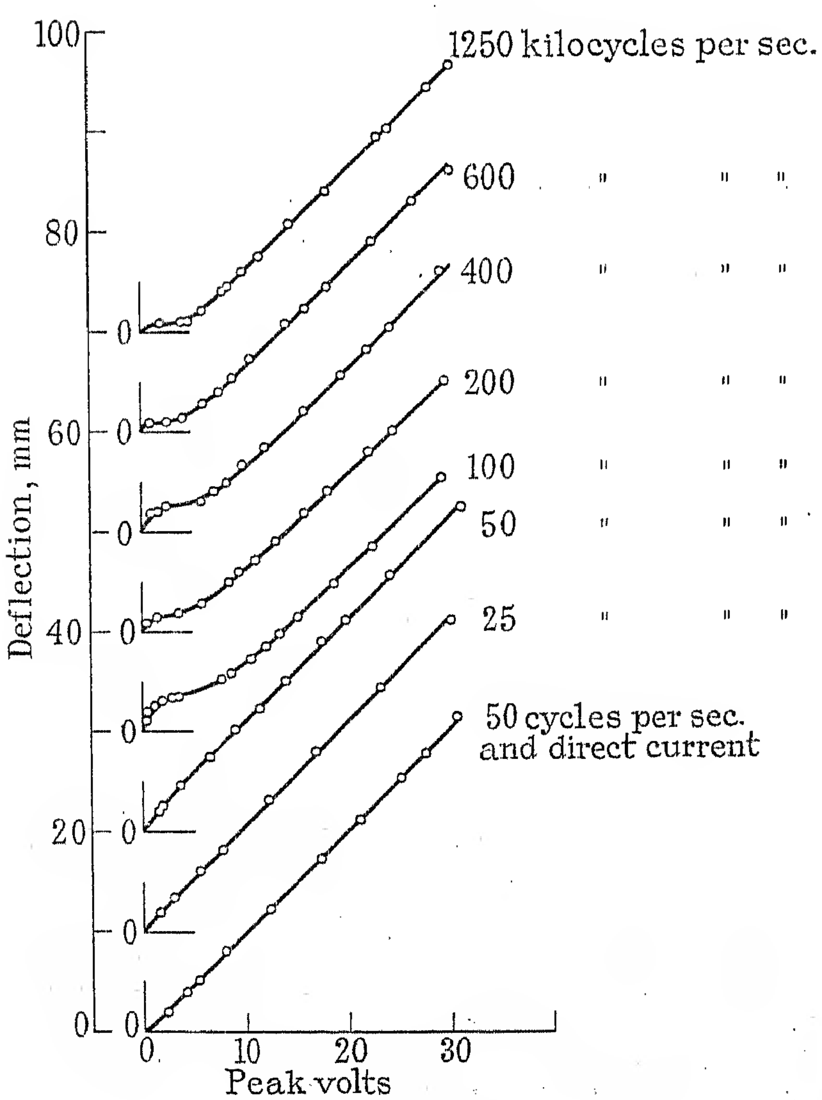

FIG. 7.—Argon tube.
$E_a = 500$ V. $I_a = 38\ \mu$A.

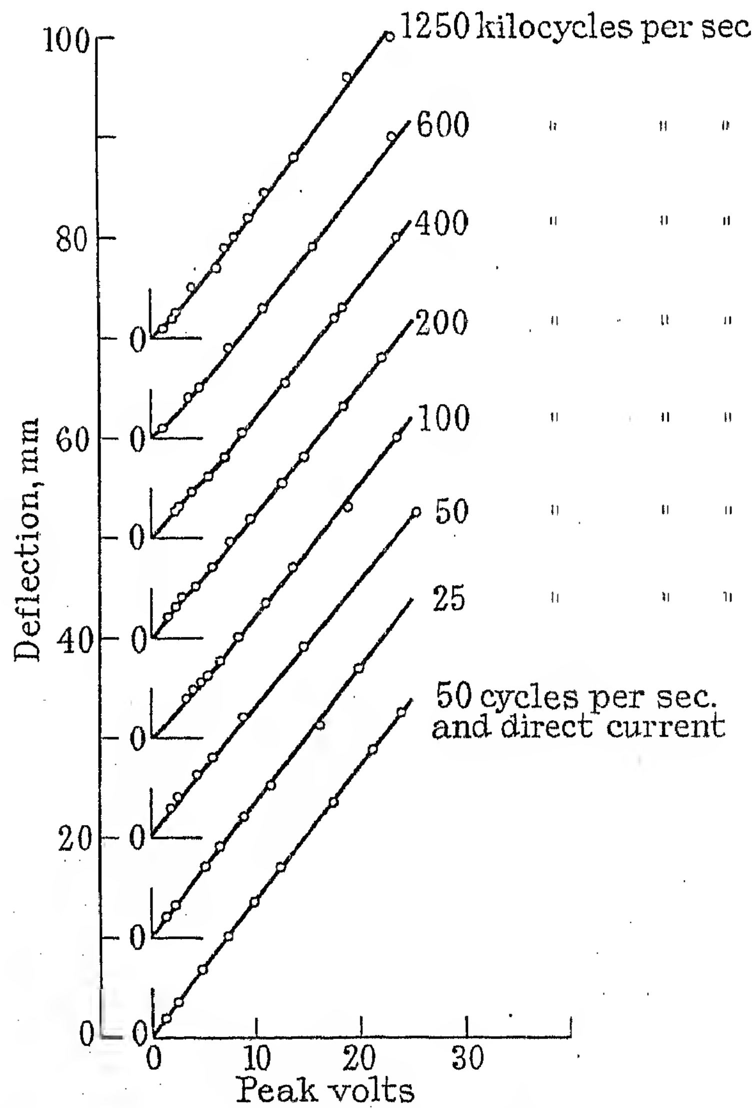

FIG. 9.—Hydrogen tube.
$E_a = 500$ V. $I_a = 50\ \mu$A.

paper stuck on the end of the screen. Although the oscillator output was very nearly sinusoidal, any possible errors due to unsymmetrical half-cycles were avoided by measuring only the deflection due to the positive half-cycle, the peak of which was indicated by the voltmeter. "Pick-up" by the oscillograph direct from the oscillator was minimized by suitable spacing between them; loss in the leads was avoided in the same manner.

Figs. 3, 4, and 5, show the results obtained for helium at a pressure of $9 \cdot 6 \times 10^{-3}$ mm of mercury, while Figs. 6, 7, and 8, show the effect of using argon at 8×10^{-3} mm under the same conditions. Fig. 9 was obtained for hydrogen at 3×10^{-3} mm, to show the decrease in the effect due to the use of this gas of lower molecular weight. Figs. 10A and 10B are replotted from the results for helium and argon for purposes of comparison, and they readily show the error which would result from using a calibration on direct current or low-

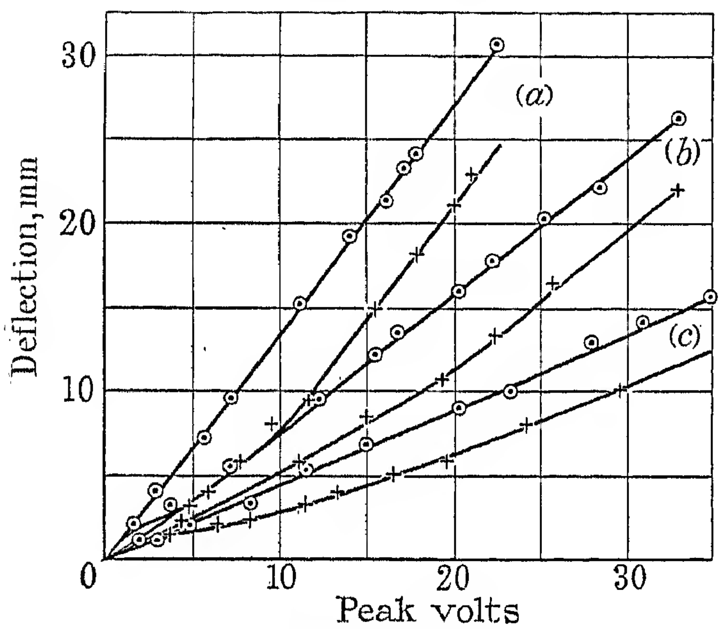

FIG. 10A.—Helium: comparison of d.c. and 1 250-kilocycle characteristics.

(a) 300 V. (b) 500 V. (c) 900 V.

——o——o—— Direct current.

——x——x—— 1 250 kilocycles per sec.

frequency alternating current for measurements made at $1 \cdot 25 \times 10^6$ cycles per sec.

A remarkable fact about some of these graphs is that an increased sensitivity is noticeable near the origin. The deflection appears to increase rapidly at first, then remain constant for the next increase in voltage, and next to merge into a uniform rise with voltage. This effect is particularly noticeable with helium, and masks the origin distortion to some extent. From the appearance of the fluorescent line when making these measurements it seems probable that this abnormally high sensitivity is caused by variation in the size of the spot due to failure of ionic focusing; the difference between the slopes of the characteristics in some cases may also be due to the same cause.

From these characteristics it is evident that a d.c. calibration of an oscillograph can be used up to 50 kilocycles per sec. but that considerable error will be introduced if measurements at higher frequencies are attempted. This, of course, applies also to the recording of rapid transients, except that since their periodic time would be unknown a calibration would be somewhat difficult.

According to calculations by Hollmann,* the error introduced at 10^6 cycles per sec. by the finite time of passage of the electrons along the axis between the deflecting plates is only $0 \cdot 02$ per cent, so that, within the range which is considered here, variations of sensitivity with frequency are entirely due to the presence of the focusing gas.

The present results for variation of origin distortion with frequency agree fairly well with those of Heimann,† although a less marked origin distortion is shown in the present results owing to the use of a lower gas pressure. The authors do not agree with Heimann in finding a difference in sensitivity between direct current and 50-cycle alternating current.

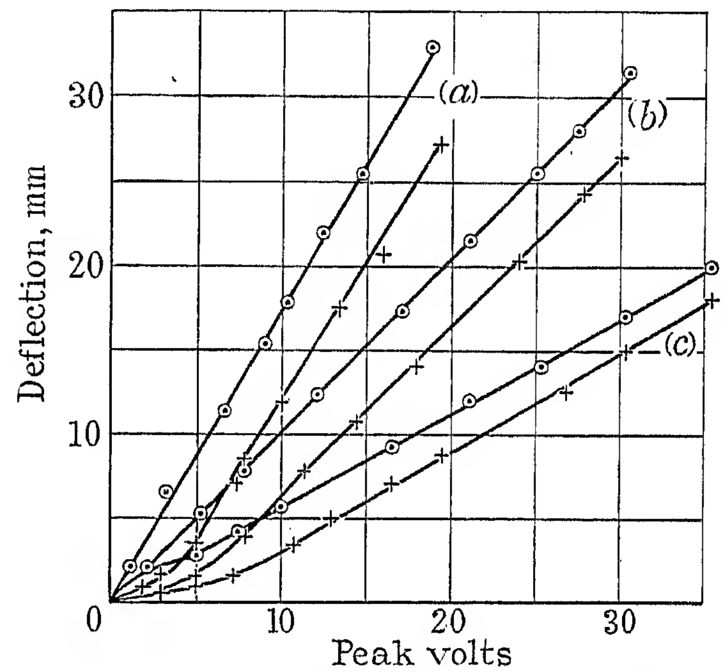

FIG. 10B.—Argon: comparison of d.c. and 1 250-kilocycle characteristics.

(a) 300 V. (b) 500 V. (c) 900 V.

——o——o—— Direct current.

——x——x—— 1 250 kilocycles per sec.

(5) GAS FOCUSING.

Experimental work on this effect has been published by many investigators; it was first studied by Johnson,‡ and more recently work has been published by von Ardenne§ and Richter.|| Mathematical theories have been developed by Engel¶ and Scherzer.** In the present paper the authors give the results of an experimental study of the dependence of ionic focusing on deflecting frequency.

As the electrons in the beam pass down the tube some of them will collide with gas molecules and, by removing electrons, form free positive ions in the path of the beam. If the transverse thermal velocity of the positive ions is sufficiently small they will remain in the beam for an appreciable time after formation and will attract the beam electrons radially inwards. Initial divergence in the beam, and subsequent divergence due to mutual repulsion between the electrons, can thus be neutralized.

For a given initial divergence and electron velocity,

* See Bibliography, (5). † *Ibid.*, (6). ‡ *Ibid.*, (8).
§ *Ibid.*, (7). || *Ibid.*, (10). ¶ *Ibid.*, (11). ** *Ibid.*, (12).

therefore, a definite density of positive ions is required in the beam in order to bring it to a minimum diameter at the fluorescent screen. The ion density in a stationary or slowly moving beam will depend upon (*a*) the beam current, (*b*) the gas pressure, and (*c*) the efficiency of ionization of the electrons. Variation of any of these factors will therefore affect the focusing conditions.

With regard to (*a*) and (*b*), the ionic density will evidently depend upon the beam current and the gas pressure, since these are respectively proportional to the density of the ionizing electrons, and of the gas to be ionized. As regards (*c*), the efficiency of ionization of an electron in a gas is defined as the number of positive ions formed per electron per centimetre of path at 0° C. and 1 mm of mercury. The variation of the efficiency for the three gases hydrogen, helium, and argon, is given in Table 7 for various anode voltages. The values for hydrogen were obtained by Tate and Smith,* and those for helium by Smith.†

From these values of the ionization efficiency it can be seen that to obtain a given ionic density with each of the three gases, different beam currents would be necessary if the gas pressure and the anode and Wehnelt-cylinder potentials were the same in each case.

TABLE 7.

Accelerating voltage	Efficiency of ionization		
	Hydrogen	Helium	Argon
volts			
300	2·03	1	8·5
500	1·4	0·7	6·2
700	1·09	0·5	4·3
900	0·9	0·4	4

Richter‡ has worked with A.E.G. tubes, using fixed anode voltage and cathode-heater current with various focusing gases and pressures; he concludes that the gas pressure necessary for correct focus depends upon the probability of ionization for the focusing gas.

When the beam is suddenly moved to a new position in the gas it will take a finite time for the ion density to build up to the required value, this time depending, as has been indicated, upon the rate of formation of ions. If the beam has a continuous rapid transverse motion the ions will never build up the required density since the beam will constantly be changing its position in the gas, so that fewer electrons will travel along any one straight path. The focus of the beam will thus become less intense, since the beam will tend to focus beyond the fluorescent screen. At certain frequencies electrons will be drawn away from the beam by a column of ions formed along a line from which the beam has just moved by a little more than its radius; this probably accounts for the lengthening of the spot observed at some frequencies.

Failure of focus will first occur in the part of the beam near the screen, since the transverse speed is greatest here. The trace on the screen will then become progressively less sharp as the frequency is raised and the decreased ion density spreads down the beam; the component electrons of the beam will then travel somewhat at random owing to the presence of stray positive space charges.

* See Bibliography, (13). † *Ibid.*, (14). ‡ *Ibid.*, (10).

It can be seen in Figs. 17, 18, and 19 (Plate 2) that above 200 kilocycles per sec. the quality of focus does not vary much with frequency. Argon retains a much sharper focus at high frequencies than either helium or hydrogen, which behave similarly, hydrogen being rather the better of the two. The decreased intensity of the trace is due to the smaller density of bombardment of the fluorescent material at the high writing speed.

When the beam current is increased the ion density increases and the focus becomes sharper, as is seen in Figs. 20, 21, and 22. The end of the trace in each case continues to be irregular; and in obtaining the sensitivity measurements (given in the previous section) by reading to the end of the trace, a different focusing condition is necessary to give the optimum end sharpness from that shown in the photographs indicating the best line sharpness.

Figs. 23 and 24 show the effect of decreasing the frequency when the focus is fixed at the optimum for 1 250 kilocycles per sec.; the beam then focuses before reaching the screen.

Thus it will be clear from these photographs that the tube requires refocusing every time a change is made in the deflecting frequency, once this frequency is high enough to cause an appreciable decrease in the ion density in the beam.

These results do not agree with the observation by von Ardenne* of a greatly increased frequency range when using hydrogen instead of argon.

(6) CONCLUSION.

The actual electrostatic sensitivity of a gas-focused cathode-ray oscillograph is about 30 per cent higher than that calculated from simple theory, the difference being possibly due to the effect on the beam of a negative charge on the insulated fluorescent screen.

It is evidently desirable to modify the design of gas-focused oscillographs by lining the inside of the glass envelope, including the fluorescent screen, with some form of conductor and anchoring its potential. The principal advantages resulting from this would be: (*a*) An increased brightness of the fluorescent figure. (*b*) Ability to operate the tube at a lower anode voltage when a higher sensitivity is required. (*c*) Certainty as to the actual sensitivity of the tube. (*d*) Complete isolation of the tube from external electrostatic fields.

Origin distortion necessarily accompanies the use of gas focusing when deflection is effected by means of simple condenser plates. For a given degree of focusing the origin distortion at low frequencies is independent of the nature of the gas used, since the focusing and the distortion depend upon the same properties of the gas. A considerable increase in origin distortion occurs at deflecting frequencies above 50 kilocycles per sec., and this increase depends upon the atomic weight of the focusing gas. The advantage of using a light focusing

* See Bibliography, (7).

gas to obtain smaller variation of sensitivity with frequency is clearly shown by the measurements described in this paper.

The three gases tested are placed in order of ionization efficiency with regard to quality of focus at high deflecting frequency, argon being better than either hydrogen or helium. It is evidently possible to compensate largely for the loss of focus by suitable increase of the beam current, although when this has been done the ends of the trace are still irregular.

This research has been rendered possible largely owing to the fact that Messrs. A. C. Cossor, Ltd., have extended many facilities to the authors, who are glad to record here their thanks. To Mr. L. H. Bedford the authors wish to express their warmest thanks for many helpful suggestions and for the trouble which he has taken in preparing the special tubes required in the course of this research.

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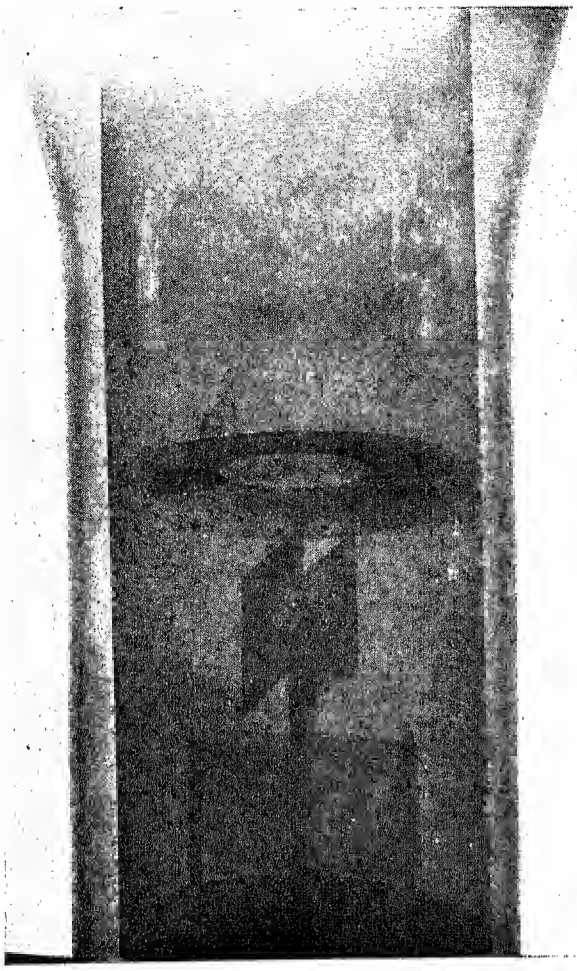

FIG. 11.—X-ray photograph used for measurement of distance between deflecting plates.

FIG. 12.—Appearance of an a.c. deflection line when a local negative charge has formed on the fluorescent screen with a low anode voltage.

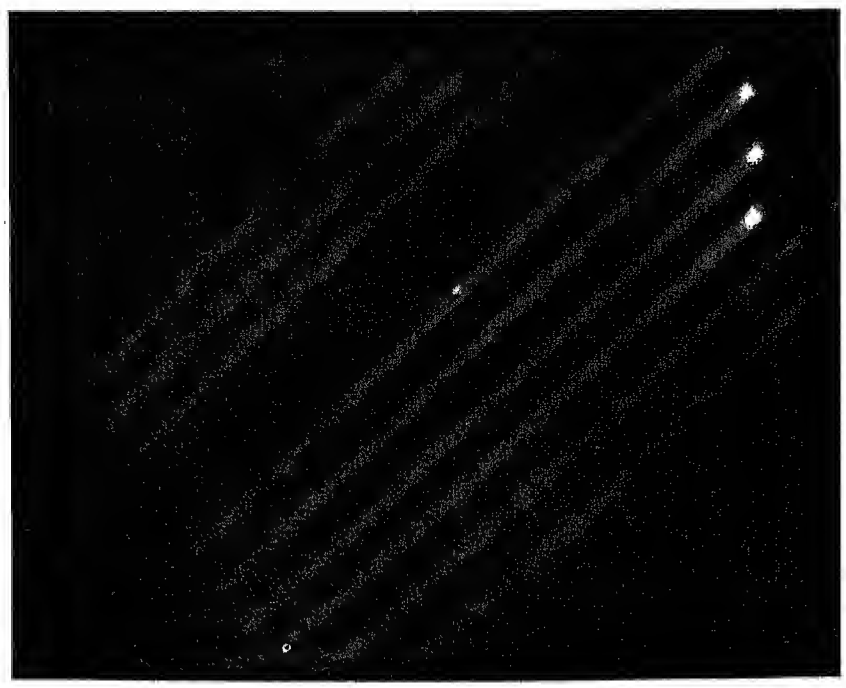

FIG. 13.—Hydrogen tube. Pressure $8 \cdot 5 \times 10^{-5}$ mm of mercury. Showing loss of focus with disappearance of origin distortion.

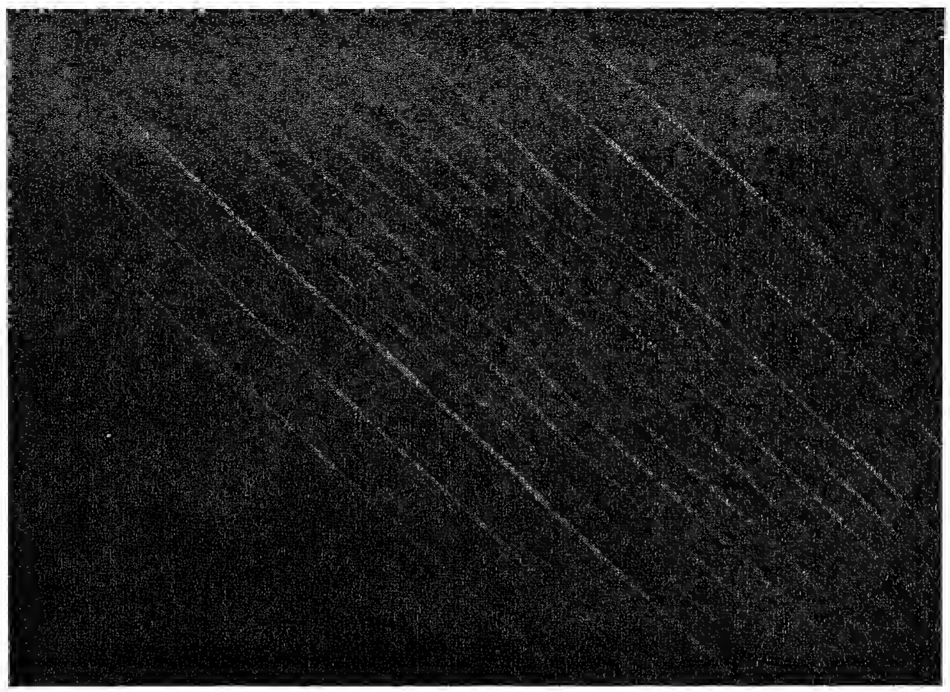

FIG. 14.—Hydrogen tube. Pressure 2×10^{-4} mm of mercury. Focus improving as origin distortion increases.

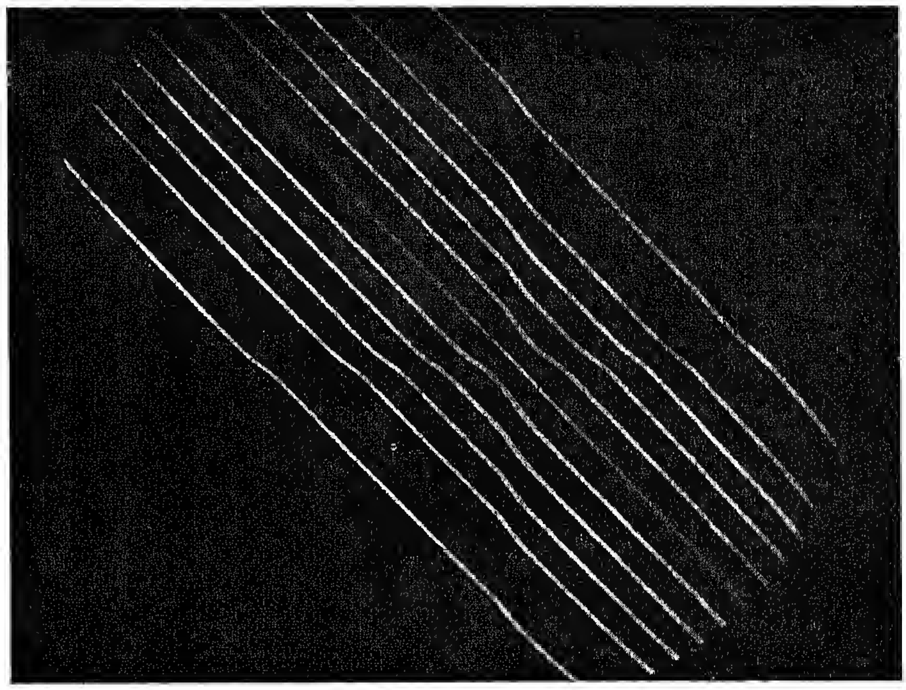

FIG. 15.—Hydrogen tube. Pressure 2×10^{-3} mm of mercury. D.C. steps $\pm$ 5, 10, 15, 20, 30.

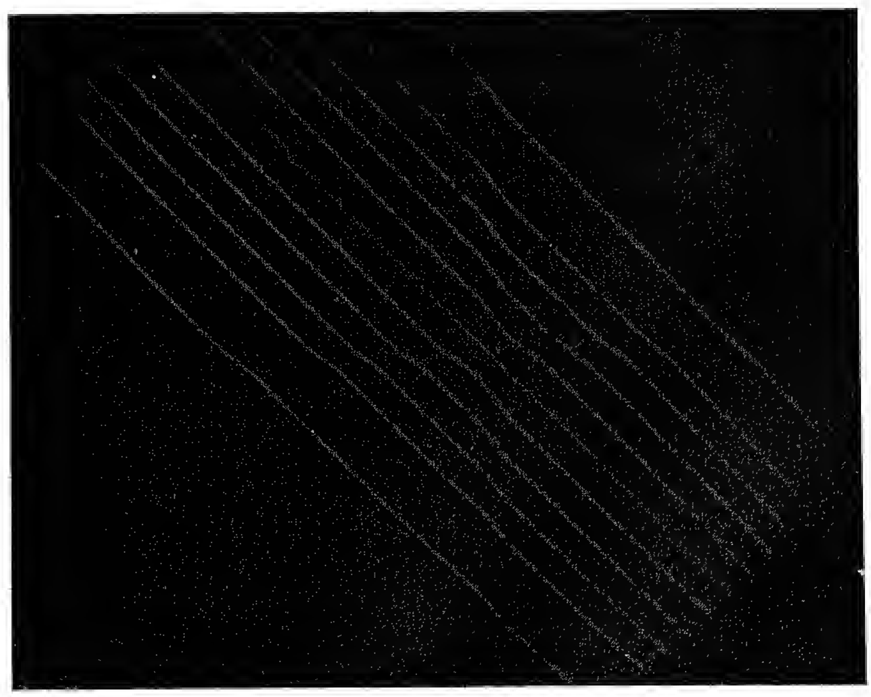

FIG. 16.—Hydrogen tube. Pressure 6×10^{-3} mm of mercury. D.C. steps $\pm$ 5, 10, 15, 20, 30.

Plate 2.

[NOTE.—In Figs. 17 to 24 the numbers above each Figure denote the deflecting frequencies in kilocycles per sec.]

25 50 100 200 400 600

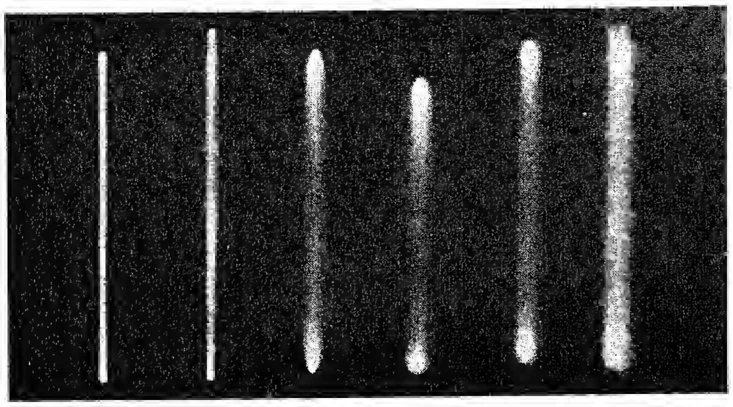

FIG. 17.—Hydrogen-focused tube.
$I = 35\ \mu A$. $E = 500$ V.

25 50 100 200 400 600 1 250

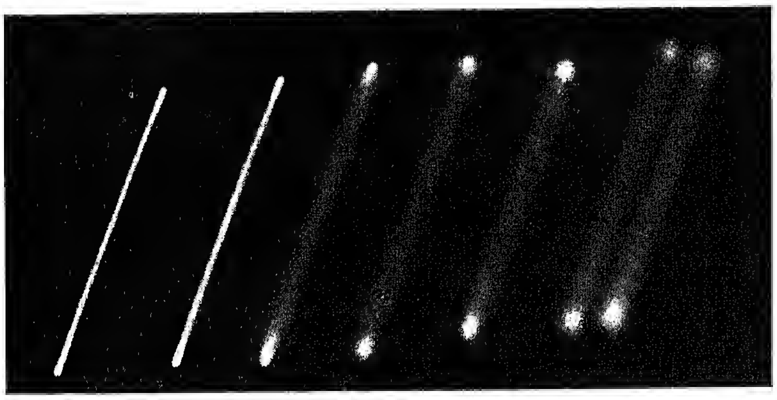

FIG. 18.—Helium-focused tube.
$I = 42\ \mu A$. $E = 500$ V.

25 50 100 200 400 600 1 250

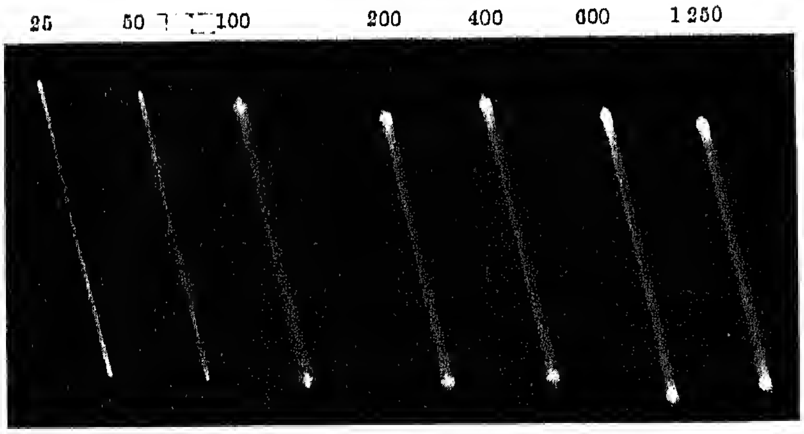

FIG. 19.—Argon-focused tube. $I = 72\ \mu A$. $E = 500$ V.

25 50 100 200 400 600 1 250

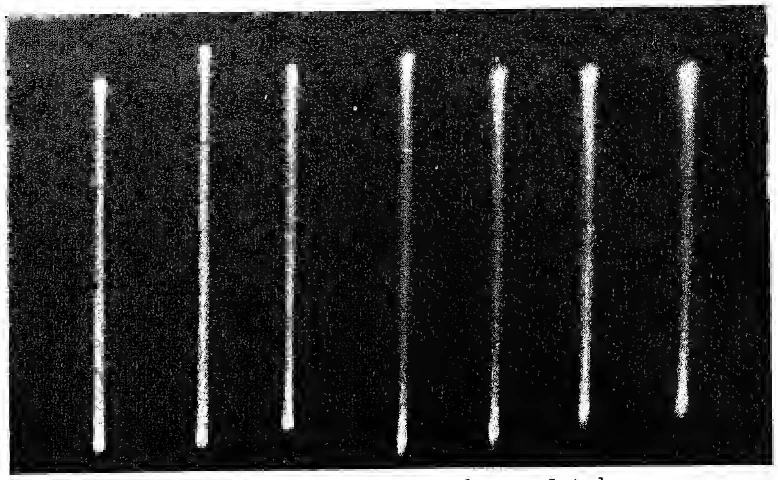

FIG. 20.—Hydrogen-focused tube.
$I = 35$–$56\ \mu A$. $E = 500$ V.

25 50 100 200 400 600 1 250

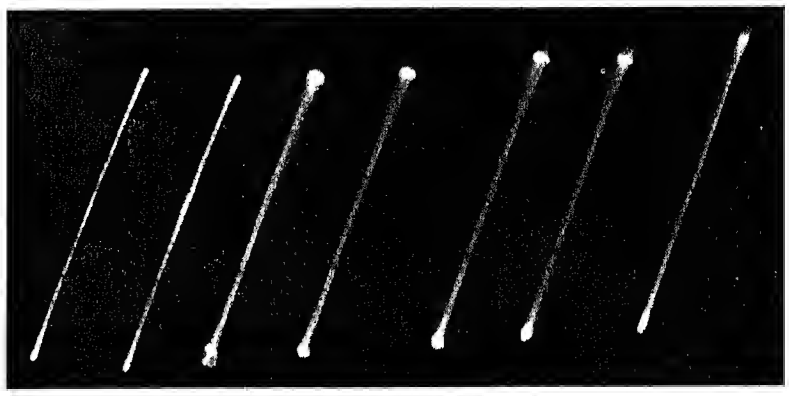

FIG. 21.—Helium-focused tube.
$I = 42$–$104\ \mu A$. $E = 500$ V.

25 50 100 200 400 600 1 250

FIG. 22.—Argon-focused tube. $I = 72$–$95\ \mu A$. $E = 500$ V.

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25 50 100 200 400 600 1 250

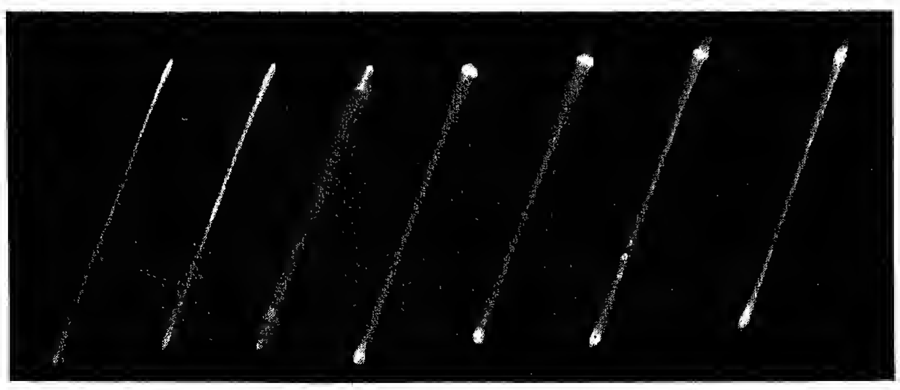

FIG. 23.—Helium-focused tube. $I = 104\ \mu A$. $E = 500$ V.

25 50 100 200 400 600 1 250

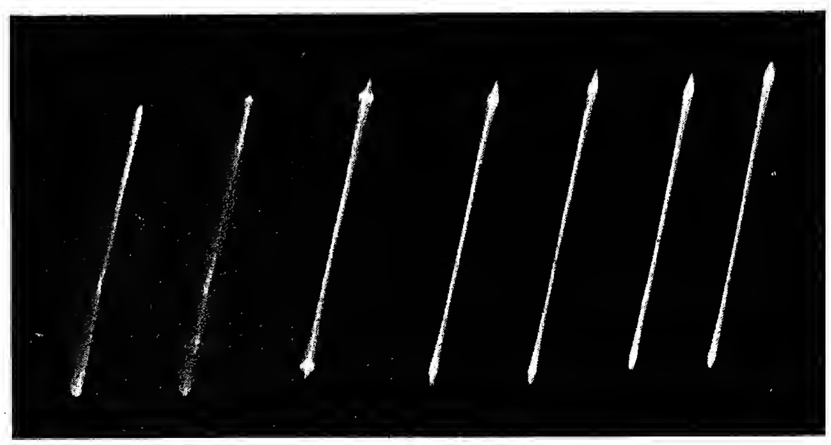

FIG. 24.—Argon-focused tube. $I = 95\ \mu A$. $E = 500$ V.

THE INHERENT INSTABILITY OF SYNCHRONOUS MACHINERY.*

By J. C. Prescott, D.Eng., Associate Member, and J. E. Richardson, Ph.D., Graduate.

(*Paper first received* 1st *August, and in final form* 5th *December*, 1933.)

Summary.

An instability in synchronous machinery which cannot be assigned to variations in the load torque or driving torque is frequently experienced. This instability was shown by Hopkinson to be due to a negative damping coefficient which can, under certain circumstances, exceed the positive damping inherent in the machine and apparatus to which it may be connected. If this condition be achieved then any displacement from the normal running position will result in a sinusoidal vibration (about this position), increasing exponentially, until the machine either falls out of step, or (the coefficient of negative damping becoming modified by the effects of increasing displacement) settles down to a steady oscillation.

In the first part of this paper the work of Rosenberg and Kapp is recapitulated to demonstrate the possibility of harmonic vibrations (about the normal running position) of alternators working in parallel. An expression is next derived for the damping coefficient of a synchronous machine in which the cross-magnetizing and demagnetizing coefficients are equal, and in which no damping grids are fitted. Some of the effects of armature reaction are ignored in this case. The conditions for instability are investigated.

The theory is next extended to the case of a machine having unequal coefficients of cross-magnetization and demagnetization, and the effects of armature reaction are considered in greater detail. An expression for the damping torque is derived, and the conditions for negative damping are again investigated.

Damping grids are then assumed to be fitted to the poles and in the interpolar spaces of the unsymmetrical machine last considered, and an expression for the damping torque is arrived at in which the armature resistance is as a first approximation neglected. This expression is compared with the torque equation of an induction motor to show how, upon analogy, the resistance of the grids for a damper of given performance may be calculated. An expression is also derived by which the negative damping due to the armature resistance may be assessed.

In the section of the paper devoted to the consideration of experimental work, measurements are described which were undertaken to confirm the formulæ obtained from mathematical analysis, and it is shown that fairly good agreement can be obtained between the measured and calculated magnitudes.

The Appendices are concerned with the further elucidation of certain points in the mathematical treatment, and with the consideration of the positive damping inherent in synchronous machines, and in certain types of apparatus to which they may be mechanically connected.

List of Symbols.

E' = effective voltage per phase.

E = maximum voltage per phase (in Appendix III = d.c. armature voltage).

* The Papers Committee invite written communications, for consideration with a view to publication, on papers published in the *Journal* without being read at a meeting. Communications (except those from abroad) should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

List of Symbols—*continued*.

I_1' = effective power current per phase.

I_1 = maximum power current per phase.

I_2' = effective wattless current per phase.

I_2 = maximum wattless current per phase.

I_1 and I_2 in Appendix III = components of fluctuating armature current in d.c. machine.

Φ_0 = maximum flux linkages per phase.

Φ = instantaneous value.

κ = flux linkages (excluding leakage) produced per phase in the coils encircling the polar axis for unit wattless current in all phases. (In Appendix III = slope of magnetization curve.)

λ = flux linkages (excluding leakage) produced per phase in the coils encircling the interpolar space for unit power current in all phases.

l_a = leakage-flux linkages per phase of the armature, for unit armature current (armature leakage self-inductance).

L_a = total flux linkages (including leakage) in the armature about the polar or interpolar axis for unit current encircling these axes (polar and interpolar self-inductance). (In Appendix III = self-inductance of d.c. armature.)

L_p, L_s, L_x in Appendix III = self-inductance of shunt field, series field, and external circuit, of a d.c. machine.

l_p, l_q = leakage flux linkages per pole per phase of the polar and interpolar dampers respectively for unit current in the dampers (damper leakage self-inductance).

L_p, L_q = total flux linkages per pole per phase of the polar and interpolar dampers respectively for unit damper current (damper self-inductance).

R_a = ohmic resistance of armature per phase (in Appendix III = resistance of d.c. armature).

R_p, R_q = ohmic resistances of polar and interpolar dampers per pole per phase.

R_p, R_s, R_x, in Appendix III = resistance of shunt field, series field, and external circuit, of a d.c. machine.

N = number of armature turns per pole per phase (the damper winding is assumed to have 1 turn per pole per phase).

n = number of phases on alternator.

n' = speed of rotation, in revolutions per sec.

$\bar{\omega}$ = speed of rotation, in radians per sec.

$\omega = 2\pi \times$ (frequency of supply).

$\Omega = 2\pi \times$ (frequency of phase swing).

u = linear velocity of turbine blades (Appendix IV).

v = steam velocity (Appendix IV).

α = nozzle angle (Appendix IV).

LIST OF SYMBOLS—*continued.*

θ = angular displacement of rotor, in mechanical radians.
T = periodic time.
$\mathbf{T}_d'$ = total damping torque.
$\mathbf{T}_d$ = damping torque for unit angular velocity.
$\mathbf{T}_s$ = synchronizing torque for unit angular displacement.
$\mathbf{I}$ = moment of inertia, in kg-m^2.
m = mass of steam per sec., in kg (Appendix IV).
g = 9·81 metres per sec. per sec.
X = equivalent armature reactance.

All flux linkages are measured in units of 100 megaline turns. A "power" current is taken to be in phase and a "wattless" current in quadrature with the e.m.f. generated by the field flux.

Conventions as to Signs.

For a generator, I_1 is positive; I_2 is positive for a lagging current and negative for a leading current.

For a motor, I_1 is negative; I_2 is positive for a leading current and negative for a lagging current.

A damping torque which opposes motion is considered to be positive.

INTRODUCTION.

The stability of synchronous machines, as affected by a periodic impulse externally imposed, was investigated by Rosenberg* and by Kapp,† who, by an approximate calculation, showed the effect of resonance between the natural free period of a machine and the periodic time of the impulse. They demonstrated the fact that parallel operation would be difficult or impossible if this condition of resonance existed, and designated the operation "unstable" in these circumstances.

In 1903, Hopkinson‡ developed more accurate formulæ for the natural free period of an alternator, in which he took account of armature reaction. He treated the problem of instability due to resonance, but also demonstrated that instability might occur even without the external periodic impulse (if certain conditions were fulfilled), owing to negative damping, which, he showed, might exist in synchronous machines. This phenomenon was again discussed by Dreyfus§ in 1911, and quite recently by Wennerberg.||

The solution of the problem involves a rather elaborate mathematical treatment, but in view of the possible troubles that may arise when interlinking is undertaken on a large scale, and when synchronous motors are used in timing devices, the present authors have been encouraged to develop a modified theory, and, gleaning in the field reaped by Hopkinson, Dreyfus, and Wennerberg, to re-state the problem, with the help of such formal mathematics as is in general use among engineers.

APPROXIMATE THEORY: THE POSSIBILITY OF SIMPLE HARMONIC VIBRATIONS.

The theory, in its simplest form, first considers the retarding torque brought into operation when the rotor of an alternator (or synchronous motor) is displaced forward from its normal running position by an angle θ. The angular displacement of the vector of back-e.m.f. E' is assumed to be equal to the angular displacement of the rotor multiplied by the number of pole pairs p, on the machine, in which case, if θ is small, the extra voltage $E'p\theta$ is available for circulating the synchronizing current. Since the resistance of the armature windings is small compared with their effective reactance, this current is nearly in quadrature with $E'p\theta$ and thus in phase or antiphase with E'.

Thus the synchronizing power is given by $p_s = E'^2p\theta/X$ watts per phase, where E' = open-circuit e.m.f. (phase) and X = equivalent reactance in the path of the synchronizing current. The synchronizing torque for a machine having n phases and working on a supply of frequency f is given by

$$\mathbf{T}_s' = \frac{nE'^2p^2}{2\pi fgX}\theta \text{ kg-m},$$

which is directly proportional to θ.

Since the rotor possesses mass, the criteria for simple harmonic motion are satisfied, and the equation of motion will be

$$\frac{\mathbf{I}}{g}\frac{d^2\theta}{dt^2} + \mathbf{T}_s\theta = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

whence

$$\theta = \theta_0 \cos(\Omega t + \epsilon) \quad . \quad . \quad . \quad . \quad (2)$$

where $\Omega = \sqrt{(\mathbf{T}_s g/\mathbf{I})}$, θ_0 and ϵ are constants, $\mathbf{I}$ = moment of inertia of the machine in kg-m^2, and $\mathbf{T}_s = nE'^2p^2/(2\pi fXg)$ kg-m; from which the natural free period is obtained in the form $T = 2\pi\sqrt{[\mathbf{I}/(\mathbf{T}_s g)]}$ seconds. Thus if an external impulse of a periodic time approximating to this be applied to the machine, partial resonance will occur, and the operation will be unstable.

The approximate theory demonstrates the possibility of the occurrence of simple harmonic vibration in the rotor of an alternator, but the theory is incomplete in that it neglects the effects of damping and some of the effects of armature reaction.

EXTENDED THEORY.

Case 1.—*The Damping Coefficient of a Symmetrical Machine.**

Let it be assumed in accordance with equation (2) that the rotor, after an initial disturbance, executes simple harmonic vibrations which can be represented by $\theta = \theta_0 \cos \Omega t$, where θ_0 = maximum angular displacement (radians), and $\Omega = 2\pi \times$ (natural frequency of alternator). Let the armature current be given by

$$i = -I_1 \cos \omega t - I_2 \sin \omega t$$

where $\omega = 2\pi \times$ (frequency of supply), and the flux linkages in the armature coils in units of weber-turns (1 weber = 100 megalines) are Φ per phase in the state of undisturbed motion; also $\Phi = \Phi_0 \sin \omega t$, Φ_0 being the maximum value of the flux linkages.

If the rotor has one pair of poles and is deflected by the angle θ, then the flux linkages in the disturbed state will be given by $\Phi_1 = \Phi_0 \sin(\omega t + \theta)$. If θ is so small

* *Journal I.E.E.*, 1909, vol. 42, p. 524.
† "Dynamo-electric Machines for Direct and Alternating Currents." (J. Springer, Berlin, 1904), p. 401.
‡ *Proceedings of the Royal Society*, 1903, vol. 72, p. 235.
§ *Schweizerische Elektrotechnischer Verein Bulletin*, 1926, vol. 17, p. 295.
|| *A.S.E.A. Journal*, 1929, vol. 6, p. 61.

* A machine in which $\kappa = \lambda$.

as to make $\sin\theta \approx \theta$ and $\cos\theta \approx 1$, the increment of flux due to the disturbance will be

$$\begin{aligned}\delta\Phi &= \theta\Phi_0 \cos\omega t \\ &= \tfrac{1}{2}\theta_0\Phi_0[\cos(\omega+\Omega)t + \cos(\omega-\Omega)t]\end{aligned}$$

and the voltage per phase generated by this flux will be

$$\begin{aligned}\delta v &= -d(\delta\Phi)/dt \\ &= \tfrac{1}{2}\theta_0\Phi_0[(\omega+\Omega)\sin(\omega+\Omega)t + (\omega-\Omega)\sin(\omega-\Omega)t]\end{aligned}$$

The current which will circulate due to this voltage will be

$$\delta i = \frac{\Phi_0\theta_0}{2}\left\{\frac{(\omega+\Omega)\sin[(\omega+\Omega)t-\alpha]}{Z_1} + \frac{(\omega-\Omega)\sin[(\omega-\Omega)t-\beta]}{Z_2}\right\}$$

where

$$Z_1 = [R_a^2 + L_a^2(\omega+\Omega)^2]^{\frac{1}{2}}, \quad Z_2 = [R_a^2 + L_a^2(\omega-\Omega)^2]^{\frac{1}{2}},$$

$\cos\alpha = R_a/Z_1$, and $\cos\beta = R_a/Z_2$.

R_a and L_a are respectively the resistance and self-inductance* in the path of the current.

This composite current may be split up into four parts: thus

$$i_1' = \tfrac{1}{2}\Phi_0\theta_0\left[\frac{\omega+\Omega}{Z_1}\cos\alpha\sin(\omega+\Omega)t\right]$$

in phase with $\frac{1}{2}\Phi_0\theta_0(\omega+\Omega)\sin(\omega+\Omega)t$; and

$$i_1'' = -\tfrac{1}{2}\Phi_0\theta_0\left[\frac{\omega+\Omega}{Z_1}\sin\alpha\cos(\omega+\Omega)t\right]$$

in quadrature with it; and similarly for the currents of the lower frequency, i_2' and i_2''.

Thus δi may be written $\delta i = i_a + i_b$, where

$$\begin{aligned}i_a &= i_1' + i_2' \\ &= \tfrac{1}{2}\Phi_0\theta_0 R_a\left[\frac{\omega+\Omega}{Z_1^2}\sin(\omega+\Omega)t + \frac{\omega-\Omega}{Z_2^2}\sin(\omega-\Omega)t\right]\end{aligned}$$

and

$$\begin{aligned}i_b &= i_1'' + i_2'' \\ &= -\tfrac{1}{2}\Phi_0\theta_0 L_a\left[\frac{(\omega+\Omega)^2}{Z_1^2}\cos(\omega+\Omega)t + \frac{(\omega-\Omega)^2}{Z_2^2}\cos(\omega-\Omega)t\right]\end{aligned}$$

Expanding the trigonometrical functions, and rearranging, we may write

$$\begin{aligned}\delta i = A\cos\omega t\sin\Omega t &+ A'\cos\omega t\cos\Omega t \\ &+ B\sin\omega t\sin\Omega t + B'\sin\omega t\cos\Omega t\end{aligned}$$

$$\text{where}\quad \left.\begin{aligned}A &= \Phi_0\theta_0 R_a\Omega(R_a^2 - L_a^2\omega^2)/Z^4, \\ A' &= -\Phi_0\theta_0 L_a\omega^2(R_a^2 + L_a^2\omega^2)/Z^4, \\ B &= 2\Phi_0\theta_0 R_a^2 L_a\omega\Omega/Z^4, \\ B' &= \Phi_0\theta_0 R_a\omega(R_a^2 + L_a^2\omega^2)/Z^4,\end{aligned}\right\}\begin{array}{l}\text{If terms in } \Omega^2 \\ \text{be neglected,} \\ \text{in comparison} \\ \text{with those in} \\ \omega^2\end{array}$$

and $\quad Z^2 = R_a^2 + L_a^2\omega^2.$

* Thus $L_a = (l_a + \lambda) = (l_a + \kappa)$, for, as pointed out by Hay ("Alternating Currents," p. 181, footnote), the effective reactance of an alternator is practically the sum of the leakage reactance and the apparent reactance due to armature reaction.

Calculation of torque.—The torque* of a synchronous machine, in kg-m per phase, may be written

$$\mathbf{T}_q = -\frac{i}{9\cdot 81}\frac{d\Phi}{d\psi} \quad . \quad . \quad . \quad . \quad (3)$$

where i is the phase current, which is assumed to remain constant while the flux linkages in the armature coils are increased by an amount $\delta\Phi$ owing to an angular displacement $\delta\psi$ of the field system.

The torque in the steady state is therefore

$$\mathbf{T}_{q_1} = -i\frac{d}{d(\omega t)}\Phi_0\sin\omega t$$

and, in the disturbed state,

$$\mathbf{T}_{q_2} = -(i+\delta i)\frac{d[\Phi_0\sin(\omega t+\theta)]}{d(\omega t+\theta)}$$

The extra torque due to the disturbance is given by

$$\delta\mathbf{T}_q = -\Phi_0(\delta i\cos\omega t - i\theta\sin\omega t) = \mathbf{T}_d'$$

if we neglect the products of small quantities.

This expression for the torque contains terms depending on the displacement of the rotor, i.e. proportional to $\cos\Omega t$, and terms depending on the velocity of this displacement, i.e. proportional to $\sin\Omega t$. The damping terms will be those in the second series.

Thus we have, for the damping torque per phase,

$$\mathbf{T}_d' = -(A\Phi_0\cos^2\omega t + B\Phi_0\sin\omega t\cos\omega t)\sin\Omega t$$

When integrated over a complete period of swing, the second term is negligible as compared with the first; thus, taking $\sin\Omega t$ out of the bracket, the mean value of the torque, in kg-m per phase, will be nearly

$$\mathbf{T}_{d(mean)}' = -\frac{\Phi_0^2\theta_0}{2g}\left[\frac{R_a(R_a^2 - L_a^2\omega^2)}{Z^4}\right]\Omega\sin\Omega t$$

and the mean damping torque (in kg-m per phase per radian per sec.) for unit angular velocity, which will act in a direction opposite to that of the velocity, is given by

$$\mathbf{T}_d = \frac{E'^2}{g\omega^2}\left[\frac{R_a(R_a^2 - L_a^2\omega^2)}{Z^4}\right] \quad . \quad . \quad . \quad (4)$$

where E' is the effective phase e.m.f.

If the machine has n phases and p pairs of poles, the total damping torque in kg-m per radian per sec. will be

$$\mathbf{T}_d = \frac{E'^2}{g\omega^2}\left[\frac{R_a(R_a^2 - L_a^2\omega^2)}{Z^4}\right]p^2 n$$

The proof of this rather surprising result may be presented in the following simplified, but less exact, form. Consider an alternator of armature reactance $L_a\omega$ and armature resistance R_a, connected to an infinite busbar of voltage V. Let E' be the back-e.m.f. of the machine. For steady conditions let $E' = V = \Phi_0\omega/\sqrt{2}$. Now let ω be increased by the small amount $\delta\omega$. Then $\delta E' = \Phi_0\delta\omega/\sqrt{2}$. This e.m.f. is available for driving a

* See, for example, C. V. Drysdale: "Future Progress in Electrical Measuring Instruments," *Journal I.E.E.*, 1933, vol. 72, p. 367.

current the component of which, in phase with V, will be

$$\delta i = \Phi_0 R_a \delta\omega / [\sqrt{2}(R_a^2 + L_a^2\omega^2)]$$

The increment of power will be

$$\delta P = E'\delta i = \Phi_0^2 R_a \omega \delta\omega / [2(R_a^2 + L_a^2\omega^2)]$$

If $\bar{\omega}$ is the angular velocity of the rotor in (mechanical) radians per second, we have $\omega = p\bar{\omega}$. Thus

$$\delta P = \Phi_0^2 R_a p^2 \bar{\omega} \delta\bar{\omega} / [2(R_a^2 + L_a^2 p^2 \bar{\omega}^2)]$$

and the torque

$$\mathbf{T}_q = \delta P / \delta\bar{\omega} = \Phi_0^2 R_a p^2 \bar{\omega} / [2(R_a^2 + L_a^2 p^2 \bar{\omega}^2)]$$

The torque per radian per second, in kg-m, is given by

$$\mathbf{T}_d = d\mathbf{T}_q / d\bar{\omega} = \Phi_0^2 R_a p^2 (R_a^2 - L_a^2\omega^2) / [2g(R_a^2 + L_a^2\omega^2)^2]$$
$$= E'^2 R_a p^2 (R_a^2 - L_a^2\omega^2) / [g\omega^2(R_a^2 + L_a^2\omega^2)^2]$$

The equation representing the phase swing of the alternator when deflected by an angle θ from its normal running position will therefore be

$$(\mathbf{I}/g)\,(d^2\theta/dt^2) + \mathbf{T}_d d\theta/dt + \mathbf{T}_s\theta = 0 \quad . \quad . \quad (5)$$

This will have a solution, if $\mathbf{T}_s$ is large compared with $\mathbf{T}_d$, given by

$$\theta = \theta_0 e^{-bt} \cos(\Omega t + \epsilon)$$

where $b = g\mathbf{T}_d/(2\mathbf{I})$, $\Omega \approx \sqrt{(\mathbf{T}_s g/\mathbf{I})}$, and ϵ = a constant depending on the initial conditions. If b is positive, the oscillations will successively diminish, but if b is negative there will be a logarithmic increase in the value of θ.

An examination of the expression for b will show that it has a negative value for alternators of normal design, since in these the equivalent reactance is greater than the resistance. For a given value of $L_a\omega$ the maximum negative value of b occurs at $R_a = 0\cdot415\,L_a\omega$, while if R_a is fixed this maximum occurs at $L_a\omega = \sqrt{3}R_a$, both maxima being unique.

As has been said, $L_a\omega$ is in general large compared with R_a, and $\mathbf{T}_d$ becomes

$$\mathbf{T}_d = -\frac{E'^2}{g\omega^2}\left[\frac{R_a}{L_a^2\omega^2}\right]p^2 n$$

Thus the negative damping is increased with an increase in R_a and decreases rapidly with increasing $L_a\omega$. This explains why the introduction of self-inductance into the armature circuit of a machine of normal design tends to reduce instability, while the introduction of resistance has the opposite effect.* Machines of high efficiency and large self-inductance will, from this point of view, be more stable than machines with large copper loss and good regulation.

We may assess the maximum negative damping that can occur in a machine, in terms of the efficiency, in the following way. Let the ratio $L_a\omega/R_a$ remain constant at $1\cdot732$; then if x be the percentage of the rated output of the machine which is expended in supplying the losses at full load, and $x/2$ the portion due to copper loss in the armature; at unity power factor we have

$$R_a I_{FL}^2 = (x/2)E'I_{FL}$$
$$R_a = E'x/(2I_{FL})$$

where I_{FL} is the full-load power current per phase, and E' the effective phase voltage. Now for $L_a\omega/R_a = \sqrt{3}$, we have

$$\mathbf{T}_{d_{max}} = -E'^2 n p^2/(8gR_a\omega^2)$$
$$= -nE'I_{FL}p^2/(4g\omega^2 x)$$

The full-load torque of the machine is given, in kg-m, by $\mathbf{T}_{FL} = nE'I_{FL}p/(g\omega)$, and thus $\mathbf{T}_{d_{max}} = -\mathbf{T}_{FL}p/(4x\omega)$. Now $1 - x = \eta$, where η is the percentage efficiency of the machine. Thus $\mathbf{T}_{d_{max}} = -T_{FL}/[4(1-\eta)\bar{\omega}]$, where $\bar{\omega} = \omega/p$ = angular velocity of the rotor of the machine.

If we take the case of a 3-phase 4 000-kW alternator working at 1 500 r.p.m. on a 50-cycle supply with an efficiency of $97\cdot5$ per cent and a line voltage of 6 000; then, for $L_a\omega = \sqrt{3}R_a$,

$$\mathbf{T}_{d_{max}} = \frac{\mathbf{T}_{FL} \times 60}{4(1 - 0\cdot975) \times 1\,500 \times 2\pi}$$
$$= \mathbf{T}_{FL}/15\cdot7$$

If we assume the machine to be star-connected the phase current is 385 amperes, the phase resistance $0\cdot112$ ohm, and the phase reactance $0\cdot195$ ohm or $2\cdot18$ per cent. For the same machine designed to have a 20 per cent reactance, $L_a\omega = 1\cdot8$ ohms per phase and $\mathbf{T}_d = \mathbf{T}_{FL}/510$. Thus, from the point of view of stability, if $L_a\omega$ is to be greater than R_a the excess should be as great as possible.

Synchronizing torque.—The value of the torque proportional to the displacement may be obtained by selecting terms in $\cos\Omega t(\sin^2\omega t$ or $\cos^2\omega t)$ in the product $-id\Phi/dt$. This gives

$$\mathbf{T}_s' = -\Phi_0\cos\Omega t[A'\cos^2\omega t + I_2\theta_0\sin^2\omega t]$$
$$= -\cos\Omega t[-(\theta_0\Phi_0^2 L_a\omega^2/Z^2)\cos^2\omega t + \Phi_0 I_2\theta_0\sin^2\omega t]$$

and, for the mean value of the torque (in kg-m per radian per phase) opposing unit angular displacement,

$$\mathbf{T}_s = \frac{1}{g}\left(\frac{E'^2 L_a}{Z^2} - \frac{E'I_2'}{\omega}\right) \quad . \quad . \quad . \quad . \quad (6)$$

where E' is the effective phase e.m.f. and I_2' the effective wattless current. For n phases and p pairs of poles, the value of $\mathbf{T}_s$ in kg-m per radian is given by

$$\mathbf{T}_s = \left(\frac{E'^2 L_a}{Z^2} - \frac{E'I_2'}{\omega}\right)p^2 n \quad . \quad . \quad . \quad . \quad (7)$$

If the machine is operating on unity power factor, and if R_a is small compared with $L_a\omega$, this becomes $\mathbf{T}_s = E'^2 p^2 n/(2\pi f X g)$, the value obtained by Rosenberg.

Case 2.—Unsymmetrical Machine without Dampers.

This case may be considered most conveniently in relation to a polyphase machine. If in such a machine the power and quadrature components of the main

* This point is dealt with further in the section on "Experimental Work."

current are assumed to be unaffected by phase swinging, then the increment of flux linkage in the armature due to a small angular displacement θ will consist of two parts, one due to the movement of the normal field through the angle θ and one due to the change produced by the movement of the magnet system in the reluctance in the paths of the main and cross-armature flux.

The first of these is given by

$$\delta\Phi_1 = \theta(\Phi_0 - \kappa I_2)\cos\omega t + \theta\lambda I_1 \sin\omega t$$

where κ is the number of flux linkages produced in an armature coil for a unit wattless current, and λ the number of flux linkages produced in the coil for unit current in phase with the e.m.f. The increment due to

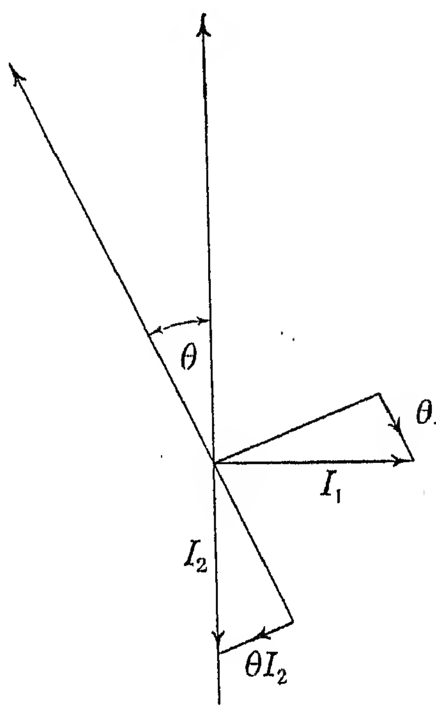

Fig. 1.

the change in reluctance of the flux paths may be calculated as follows. If in Fig. 1 the polar axis moves forward by the small angle θ while the currents I_1 and I_2 remain constant in magnitude and direction, then the increment of current surrounding the polar axis will be $-\theta I_1$ and the flux linkages produced by this will be $-\theta\kappa I_1$. Similarly an increment of current θI_2 round the cross axis will produce a flux $\theta\lambda I_2$.

We may assume, with reference to the results of the previous section, an expression for the current δi. Let this expression be $\delta i = i_1 + i_2$, where

$$i_1 = A\cos\omega t \sin\Omega t + A'\cos\omega t\cos\Omega t$$
$$i_2 = B\sin\omega t\sin\Omega t + B'\sin\omega t\cos\Omega t$$

This current δi will produce a flux κi_2 along the polar axis and λi_1 in quadrature with that axis. The total increment of flux will therefore be

$$\delta\Phi = [\Phi_0 - I_2(\kappa - \lambda)]\theta\cos\omega t - (\kappa - \lambda)I_1\theta\sin\omega t + \lambda i_1 + \kappa i_2$$

This will produce an increment of e.m.f. in the armature which must be equal and opposite to the resistance and leakage-reactance drops due to the increment of current. Thus

$$d(\delta\Phi)/dt + l_a d(\delta i)/dt + R_a\delta i = 0$$

where l_a is the leakage self-inductance of the armature and R_a the armature resistance. Introducing the values of δi and $\delta\Phi$, and equating the coefficients of the trigonometrical functions, we obtain, if we neglect Ω^2 in comparison with ω^2,

$$A = (\theta_0 R_a\Omega/Z^4)\big\{[\Phi_0 - I_2(\kappa-\lambda)][R_a^2 - (l_a+\kappa)^2\omega^2] + I_1(\kappa-\lambda)R_a\omega[(l_a+\kappa)+(l_a+\lambda)]\big\},$$
$$A' = \theta_0\big\{I_1(\kappa-\lambda)R_a\omega - [\Phi_0 - I_2(\kappa-\lambda)](l_a+\kappa)\omega^2\big\}/Z^2,$$
$$B = (\theta_0 R_a\Omega/Z^4)\big\{-I_1(\kappa-\lambda)[R_a^2 - (l_a+\lambda)^2\omega^2] + [\Phi_0 - I_2(\kappa-\lambda)]R_a\omega[(l_a+\kappa)+(l_a+\lambda)]\big\},$$
$$B' = \theta_0\big\{[\Phi_0 - I_2(\kappa-\lambda)]R_a\omega + (\kappa-\lambda)I_1(l_a+\lambda)\omega^2\big\}/Z^2,$$

where $Z^2 = R^2 + (l_a+\kappa)(l_a+\lambda)\omega^2$.

The total linkages in the armature may now be written down for the deflected position. We have

$$\Phi = (\Phi_0 - \kappa I_2)\sin\omega t - \lambda I_1\cos\omega t + \delta\Phi_1 + \lambda A\cos\omega t\sin\Omega t + \lambda A'\cos\omega t\cos\Omega t + \kappa B\sin\omega t\sin\Omega t + \kappa B'\sin\omega t\cos\Omega t + \theta_0\lambda I_2\cos\omega t\cos\Omega t - \theta_0\kappa I_1\sin\omega t\cos\Omega t$$

θ being small, this becomes

$$\Phi = (\Phi_0 - \kappa I_2)\sin(\omega t + \theta) - \lambda I_1\cos(\omega t + \theta) + \theta_0\lambda I_2\cos\omega t\cos\Omega t - \theta_0\kappa I_1\sin\omega t\cos\Omega t + \lambda A\cos\omega t\sin\Omega t + \lambda A'\cos\omega t\cos\Omega t + \kappa B\sin\omega t\sin\Omega t + \kappa B'\sin\omega t\cos\Omega t,$$

and the total armature current in the deflected position is

$$\bar{i} = -I_1\cos\omega t - I_2\sin\omega t + \delta i$$

To obtain the torque, Φ must be differentiated with respect to $(\omega t + \theta)$, and the derivative multiplied by the armature current.

This differentiation must be carried out as if all the currents were constant during the variation of $(\omega t + \theta)$. Thus in the expression for the flux $\sin\Omega t$ and $\cos\Omega t$ become constants, while the sines and cosines of ωt represent the varying projections of the flux vectors on the x and y axes for a change in $(\omega t + \theta)$. Thus the expression for the torque* becomes

$$\mathbf{T}_d = R_a\frac{[\Phi_0 - (\kappa-\lambda)I_2]^2[R_a^2 - (l_a+\kappa)^2\omega^2] + I_1^2(\kappa-\lambda)^2[R_a^2 - (l_a+\lambda)^2\omega^2]}{2Z^4 g} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and for the synchronizing torque we have

$$\mathbf{T}_s = \frac{[\Phi_0 - (\kappa-\lambda)I_2]^2(l_a+\kappa)^2\omega^2 + I_1^2(\kappa-\lambda)(l_a+\lambda)\omega^2}{2Z^2 g} - \frac{[\Phi_0 - I_2(\kappa-\lambda)]I_2 - I_1^2(\kappa-\lambda)}{2g} \quad . \quad . \quad (9)$$

* Cf. Hopkinson, who refers I_1 and I_2 to the resultant flux in the undisturbed state, and not, as is done here, to the polar axis.

which have the same general form as the expressions derived in Case 1, but, as would be expected, the amount of instability is affected by the armature current. This effect decreases, however, as the value of λ approaches that of κ, until, when equality is attained

$$\mathbf{T}_d = \Phi_0^2 R_a[R_a^2 - (l_a + \kappa)^2\omega^2]/(2Z^4 g) \quad . \quad . \quad (10)$$

In the unsymmetrical case, the increase in instability with $(l_a + \kappa)\omega > R_a$ due to an increase in Φ_0 is partly counterbalanced by the increase in the wattless component I_2, and therefore the effect of a variation in the excitation on the stability of the alternator appears less marked. Also, the instability shows an increase with load for $(l_a + \kappa)\omega > R_a$.

Case 3.—*Unsymmetrical Machine with Dampers.*

As before, the currents I_1 and I_2 will have components $-\theta I_1 \sin\omega t$ encircling the displaced position of the polar axis, and $\theta I_2 \cos\omega t$ encircling the interpolar space. Thus the increment of flux linkage in the single turns of the damper winding due to these currents will be $-\theta_0(\kappa I_1/N)\cos\Omega t$ in the line of the pole, and $(\theta_0\lambda I_2/N)\cos\Omega t$ in quadrature with the polar axis, where N is the effective number of armature turns per pole per phase. The dampers are assumed to have each one turn per pole per phase.

The increment of armature current is, as before,

$$\delta i = A\cos\omega t\sin\Omega t + A'\cos\omega t\cos\Omega t + B\sin\omega t\sin\Omega t + B'\sin\omega t\cos\Omega t,$$

and the flux linkages produced in the damper by this are $(B\kappa/N)\sin\Omega t + (B'\kappa/N)\cos\Omega t$ in the polar axis and $(A\lambda/N)\sin\Omega t + (A'\lambda/N)\cos\Omega t$ at right angles to the axis.

We now write the damper current as

$$i_q = a\sin\Omega t + a'\cos\Omega t$$

encircling the interpolar space and

$$i_p = b\sin\Omega t + b'\cos\Omega t$$

encircling the polar axis. The flux linkages produced by it in the damper are

$$(a\lambda/N^2)\sin\Omega t + (a'\lambda/N^2)\cos\Omega t$$

and

$$(b\kappa/N^2)\sin\Omega t + (b'\kappa/N^2)\cos\Omega t$$

Thus the total flux linkages in the damper are

$$\Phi_q = (\lambda/N^2)(NA + a)\sin\Omega t + (\lambda/N^2)(NA' + a' + \theta_0 I_2 N)\cos\Omega t$$

and

$$\Phi_p = (\kappa/N^2)(NB + b)\sin\Omega t + (\kappa/N^2)(NB' + b' - \theta_0 I_1 N)\cos\Omega t$$

If l_p, l_q be the leakage self-inductances of the dampers, and R_p, R_q the resistances of the dampers, then

$$d\Phi_q/dt + l_q(di_q/dt) + R_q i_q = 0 \quad . \quad . \quad (11)$$

and similarly for Φ_p.

Substituting the values of Φ_p, Φ_q, i_p, and i_q, we may solve for a, a', b, and b', by equating coefficients, and so determine Φ_p and Φ_q in terms of A, A', B, and B'. Remembering that the flux linkages in the armature are N times those in the damper, we may write the increment of armature flux due to the action of the currents, induced by phase swinging, in the armature and dampers, as

$$\begin{aligned}\delta\Phi_1 &= N(\Phi_q\cos\omega t + \Phi_p\sin\omega t)\\ &= \Phi_1\cos\omega t\sin\Omega t + \Phi_2\cos\omega t\cos\Omega t\\ &\quad + \Phi_3\sin\omega t\sin\Omega t + \Phi_4\sin\omega t\cos\Omega t^*\end{aligned}$$

where

$$\Phi_1 = \lambda[A\gamma_1 + (A' + \theta_0 I_2)\beta_1], \quad \Phi_2 = \lambda[(A' + \theta_0 I_2)\gamma_1 - A\beta_1],$$
$$\Phi_3 = \kappa[B\gamma_2 + (B' - \theta_0 I_1)\beta_2], \quad \Phi_4 = \kappa[(B' - \theta_0 I_1)\gamma_2 - B\beta_2],$$

and

$$\gamma_1 = \left(1 - \frac{\lambda L_q\Omega^2}{N^2X_1^2}\right), \qquad \gamma_2 = \left(1 - \frac{\kappa L_p\Omega^2}{N^2X_2^2}\right),$$
$$\beta_1 = \lambda R_q\Omega/(N^2X_1^2), \qquad \beta_2 = \kappa R_p\Omega/(N_2X_2^2),$$
$$X_1^2 = (R_q^2 + L_q^2\Omega^2), \qquad X_2^2 = (R_p^2 + L_p^2\Omega^2),$$
$$L_q = (\lambda/N^2) + l_q, \qquad L_p = (\kappa/N^2) + l_p,$$

and R_q, L_q, and l_q, are respectively the resistance, self-inductance, and leakage self-inductance, for the damper encircling the interpolar space; while R_p, L_p, and l_p are the same, respective, quantities for the damper encircling the polar axis.

The increment of flux linkage in the armature is

$$\delta\Phi = \theta_0(\Phi_0 - \kappa I_2)\cos\omega t\cos\Omega t + \theta_0\lambda I_1\sin\omega t\cos\Omega t + \delta\Phi_1\dagger$$

As before,

$$\frac{d(\delta\Phi)}{dt} + l_a\frac{(d\delta i)}{dt} + R_a\delta i = 0 \quad . \quad . \quad . \quad (12)$$

If we insert the values of $\delta\Phi$ and δi here, the resulting equations become very complex,‡ but a simplification may be achieved by the assumption that, as is generally the case, the effect of self-inductance is large compared with that of resistance; and we may write $\delta\Phi + l_a\delta i = 0$. Substituting here for δi and $\delta\Phi$, we obtain four equations of coefficients corresponding to the sines and cosines of $(\omega + \Omega)$ and $(\omega - \Omega)$, which may be solved to give

$$A = \theta_0\frac{\lambda\beta_1[\Phi_0 - (\kappa + l_a)I_2]}{\lambda^2\beta_1^2 + (l_a + \lambda\gamma_1)^2},$$

$$A' = -\theta_0\frac{(l_a + \lambda\gamma_1)[\Phi_0 - I_2(\kappa - \lambda\gamma_1) + \lambda^2\beta_1^2 I_2]}{\lambda^2\beta_1^2 + (l_a + \lambda\gamma_1)^2},$$

$$B = \theta_0\frac{I_1\kappa\beta_2(l_a + \lambda)}{\kappa^2\beta_2^2 + (l_a + \kappa\gamma_2)^2},$$

and

$$B' = \theta_0\frac{\kappa^2\beta_2^2 I_1 - (l_a + \kappa\gamma_2)(\lambda - \kappa\gamma_2)I_1}{\kappa^2\beta_2^2 + (l_a + \kappa\gamma_2)}.$$

* If terms in $\sin^2\Omega t$ and $\cos^2\Omega t$ are neglected.
† $(\theta_0\lambda I_2\cos\omega t - \theta_0\kappa I_1\sin\omega t)$ is included in $\delta\Phi_1$.
‡ See Appendix I.

The differential of the main flux with respect to $(\omega t + \theta)$ is

$$\frac{d\Phi}{d(\omega t+\theta)} = (\Phi_0 - \kappa I_2)\cos\omega t + \lambda I_1 \sin\omega t - \theta_0(\Phi_0 - \kappa I_2)\sin\omega t\cos\Omega t + \theta_0\lambda I_1\cos\omega t\cos\Omega t$$
$$- \Phi_1\sin\omega t\sin\Omega t - \Phi_2\sin\omega t\cos\Omega t + \Phi_3\cos\omega t\sin\Omega t + \Phi_4\cos\omega t\cos\Omega t$$

This, when multiplied by the total current, gives, for the total damping torque, if we select terms in

$$\sin\Omega t(\cos^2\omega t \quad \text{or} \quad \sin^2\omega t),$$

$$\mathbf{T}_d' = \frac{\theta_0\lambda\beta_1[\Phi_0-(\kappa+l_a)I_2]^2}{2X_q^2} + \frac{\theta_0\kappa\beta_2 I_1^2(\lambda+l_a)^2}{2X_p^2} \quad \text{(13)}$$

where $X_q^2 = \lambda^2\beta_1^2 + (l_a+\lambda\gamma_1)^2$ and $X_p^2 = \kappa^2\beta_2^2 + (l_a+\kappa\gamma_2)^2$.

Inspection of this expression shows that for $R_q = \infty$, $\beta_1 = 0$. That is to say, when the interpolar dampers have a large resistance or where such dampers are not fitted, the first term disappears and the machine experiences no damping effect when on no load.*

By introducing the values of β_1, γ_1, β_2, and γ_2, we have the torque

$$\mathbf{T}_d' = \theta_0\frac{[\Phi_0-(\kappa+l_a)I_2]^2}{2g}\left\{\frac{N^2R_q\Omega}{[l_a+N^2l_q+(N^2l_al_q/\lambda)]^2\Omega^2+[N^2R_q(l_a+\lambda)/\lambda]^2}\right\}$$
$$+ \theta_0\frac{(\lambda+l_a)^2I_1^2}{2g}\left\{\frac{N^2R_p\Omega}{[l_a+N^2l_p+(N^2l_al_p/\kappa)]^2\Omega^2+[N^2R_p(l_a+\kappa)/\kappa]^2}\right\}$$

If we put $E_1' = [\Phi_0-(\kappa+l_a)I_2]\omega/\sqrt{2}$ as the voltage in phase with I_1, and $E_2' = (\lambda+l_a)I_1\omega/\sqrt{2}$ as the voltage in phase with I_2, then

$$\mathbf{T}_d' = \frac{\theta_0E_1'^2}{\omega^2 g}\left\{\frac{N^2R_q\Omega}{[l_a+N^2l_q+(N^2l_al_q/\lambda)]^2\Omega^2+[N^2R_q(l_a+\lambda)/\lambda]^2}\right\}$$
$$+\frac{\theta_0E_2'^2}{\omega^2 g}\left\{\frac{N^2R_p\Omega}{[l_a+N^2l_p+(N^2l_al_p/\kappa)]^2\Omega^2+[N^2R_p(l_a+\kappa)/\kappa]^2}\right\}$$
$$=\frac{\theta_0E_1'^2}{\omega g}\left\{\frac{N^2R_q\sigma}{[l_a\omega+N^2l_q\omega+(N^2l_al_q\omega/\lambda)]^2\sigma^2+[N^2R_q(l_a+\lambda)/\lambda]^2}\right\}$$
$$+\frac{\theta_0E_2'^2}{\omega g}\left\{\frac{N^2R_p\sigma}{[l_a\omega+N^2l_p\omega+(N^2l_al_p\omega/\kappa)]^2\sigma^2+[N^2R_p(l_a+\kappa)/\kappa]^2}\right\} \quad \text{(14)}$$

where $\sigma = \Omega/\omega$. Comparing these two component torques with the torque on an induction motor, we see that they are identical if σ is equal to the slip.†

Extending the expression to give the torque for a machine of p pole pairs and n phases, we multiply by the factor np^2, so that ω becomes ω/p and $\sigma = \Omega p/\omega$. Thus the damping grids may be designed on analogy with the rotor of an induction motor operating at slip σ, i.e. in order that maximum torque may be developed at this value of the slip (if the damper resistance is the variable) $d\mathbf{T}_d'/dR_p = 0$, which gives

$$R_p = \frac{[l_a\omega+N^2l_p\omega+(N^2l_al_p\omega/\kappa)]\sigma}{[N^2(l_a+\kappa)/\kappa]^2} \qquad R_q = \frac{[l_a\omega+N^2l_q\omega+(N^2l_al_q\omega/\lambda)]\sigma}{[N^2(l_a+\lambda)/\lambda]^2}$$

The damping torque for unit angular velocity is found by dividing $\mathbf{T}_d'$ by $\Omega\theta_0$, which gives

$$\mathbf{T}_d = \frac{E_1^2np^2}{\omega^2 g}\left\{\frac{N^2R_q}{[l_a+N^2l_q+(N^2l_al_q/\lambda)]^2\Omega^2+[N^2R_q(l_a+\lambda)/\lambda]^2}\right\}$$
$$+\frac{E_2^2np^2}{\omega^2 g}\left\{\frac{N^2R_p}{[l_a+N^2l_p+(N^2l_al_p/\kappa)]^2\Omega^2+[N^2R_p(l_a+\kappa)/\kappa]^2}\right\} \quad \text{(15)}$$

which is a positive quantity for all possible values of the variables.

Effect of Armature Resistance.—If we retain the armature resistance in equation (12) we obtain, in addition to the terms which we have just investigated, a series of terms in $R_a\Omega$ corresponding with those obtained in equations (4) and (8), and a second series in R_a^2 which now appear for the first time. All the terms are very complex and we shall only consider here the dominant parts of them, namely those parts which include Φ_0^2.‡

* This statement will be modified later when the effect of armature resistance is included.
† STEINMETZ: "Electrical Engineering," p. 313 (edition of 1915).
‡ The complete expression will be found in Appendix I.

The torque for unit angular velocity due to the terms in $R_a\Omega$ is given by

$$\mathbf{T}_d = \frac{R_a\Omega}{\omega^2}\frac{\left[(\Phi_1 + aI_2)^2 + (cI_2)^2\right]\left[(R_a^2/\omega^2) - (b^2 + e^2)\right]}{2X_0^2\Omega}$$

where

$\Phi_1 = \Phi_0 - (l_a + \kappa)I_2$,

$a = (l_a + \lambda\gamma_1)$, $b = (l_a + \kappa\gamma_2)$, $c = \beta_1\lambda$, $e = \beta_2\kappa$, and

$$X_0^2 = \left\{\frac{R_a}{\omega^2}\left[R_a + (c + e)\Omega\right] + (ab - ce)\right\}^2 + \left[ae + bc - \frac{R_a\Omega}{\omega^2}(a + b)\right]^2$$

If R_a/ω^2 is small compared with ab, ce, ae, and bc, this reduces to

$$\mathbf{T}_d = -\frac{R_a}{2\omega^2}\left[(\Phi_1 + aI_2)^2 + (cI_2)^2\right]\frac{1}{a^2 + c^2}$$

Comparing this with the expression for the torque (equation 8), we see that $(l_a + \lambda)$ has been replaced by $(l_a + \lambda\gamma_1)$ and that terms in β have been introduced. Putting in the values of a and c in the denominator, we have

$$\mathbf{T}_d = -\frac{R_a}{2\omega^2}\left[(\Phi_1 + aI_2)^2 + (cI_2)^2\right]\frac{N^4\left\{R_q^2 + \left[(\lambda/N^2) + l_q\right]^2\Omega^2\right\}}{\Omega^2(\lambda l_a + N^2 l_a l_q + N^2 l_q\lambda)^2 + N^4\left[R_q(l_a + \lambda)\right]^2} \quad . \quad . \quad . \quad (16)$$

If the resistance of the interpolar dampers R_q is large compared with the reactance, or if these dampers are not fitted, then $\beta_1 = 0$ and the expression is identical with the first reactive term in equation (8).

If R_q is small as compared with λ/N^2 and l_q, and we assume that $N^2 l_q$ may be neglected in comparison with λ, then the expression becomes

$$\mathbf{T}_d = -\frac{R_a}{2\omega^2}(\Phi_1 + aI_2)^2\frac{1}{(l_a + N^2 l_q)^2}$$

i.e. the negative damping due to the armature resistance has been increased by the substitution in the denominator of $(l_a + N^2 l_q)^2$ for $(l_a + \lambda)^2$. The fitting of interpolar dampers of too low resistance is therefore seen to increase the instability due to armature resistance, while, as before, for values of R_a which are small compared with $(l_a + N^2 l_q)$ the (negative) damping coefficient is directly proportional to R_a.

The expression for the torque due to the terms in R_a^2 is, if we confine ourselves to those containing Φ_1^2,

$$\mathbf{T}_d = -\frac{R_a^2 e}{2\omega^2\Omega}\left[\frac{(\Phi_1 + aI_2)^2}{X_0^2}\right]$$

which may be expanded to give

$$\mathbf{T}_d = -\frac{R_a^2}{2\omega^2}(\Phi_1 + aI_2)^2\frac{1}{a^2 + c^2}\left\{\frac{N^2 R_p\kappa^2}{(l_a\kappa + N^2 l_p\kappa + N^2 l_p l_a)^2\Omega^2 + N^4\left[R_p(l_a + \kappa)\right]^2}\right\} \quad . \quad . \quad . \quad (17)$$

The factor $1/(a^2 + c^2)$ has already been investigated and shown to be dependent on the constants of the interpolar damper. The magnitude of the term in the curly brackets is, however, determined by the constants of the damper winding encircling the polar axis.

Taking the case of a machine without interpolar dampers, we have

$$\mathbf{T}_d = -\frac{R_a^2}{2\omega^2}\frac{(\Phi_1 + aI_2)^2}{(l_a + \lambda)^2}\left\{\frac{N^2 R_p\kappa^2}{(l_a\kappa + N^2 l_p\kappa + N^2 l_p l_a)^2\Omega^2 + \left[N^2 R_p(l_a + \kappa)\right]^2}\right\}$$

This gives, if we neglect l_a as compared with κ,

$$\mathbf{T}_d = -\frac{R_a^2}{2\omega^2}\frac{(\Phi_1 + aI_2)^2}{(l_a + \lambda)^2}\left[\frac{N^2 R_p}{(l_a + N^2 l_p)^2\Omega^2 + (N^2 R_p)^2}\right]$$

which for small values of $(l_a + N^2 l_p)\Omega$ is inversely proportional to $N^2 R_p$.

A machine, then, which has only dampers encircling the polar axis may exhibit an instability due to these dampers if they are of low resistance, and if the armature and damper leakage-reactances are small also. This instability is nearly proportional to the square of the main flux and will be most noticeable at no load, where, as has been shown, the positive torque due to the polar dampers is zero.

Relative magnitudes of damping torques.—By writing $I_1 = I_2 = 0$ and neglecting l_a in comparison with κ and λ, we obtain the following simplified expressions for the three damping torques which we have just considered in a machine where the resistance of the armature is small compared with its equivalent reactance.

(i) For the positive damping torque (see equation 15) we have,

$$\mathbf{T}_{d_1} = \frac{E'^2 np^2}{\omega^2 g}\left[\frac{N^2 R_q}{(l_a + N^2 l_q)^2\Omega^2 + (N^2 R_q)^2}\right]$$

If the first term in the denominator of the square bracket is small, this becomes

$$\mathbf{T}_{d_1} = \frac{E'^2 np^2}{\omega^2 g}\left(\frac{1}{N^2 R_q}\right)$$

(ii) For the negative damping torque in $R_a\Omega$ (see equation 16) we have,

$$\mathbf{T}_{d_2} = -\frac{E'^2 np^2}{\omega^2 g}\frac{R_a}{\omega^2}\left[\frac{N^4 R_q^2 + \lambda^2\Omega^2}{\lambda^2(l_a + N^2 l_q)^2\Omega^2 + (N^2 R_q\lambda)^2}\right]$$

If $\lambda\Omega$ is small compared with R_q this becomes

$$\mathbf{T}_{d_2} = -\frac{E'^2 np^2}{\omega^2 g}\left(\frac{R_a}{\lambda^2\omega^2}\right)$$

(iii) For the negative damping torque in R_a^2 (see equation 17) we have,

$$\mathbf{T}_{d_3} = -\frac{E'^2 np^2}{\omega^4 g}\frac{R_a^2}{a^2 + c^2}\left[\frac{N^2 R_p}{(l_a + N^2 l_p)^2\Omega^2 + (N^2 R_p)^2}\right]$$

For the case where there are no interpolar dampers and where $(l_a + N^2 l_p)\Omega$ is small compared with N^2R_p, we have

$$\mathbf{T}_{d_3} = -\frac{E'^2np^2}{\omega^2 g}\frac{R_a^2}{\lambda^2\omega^2}\left(\frac{1}{N^2R_p}\right)$$

For a machine operating at or near no load and unity power factor we have a positive torque due to the interpolar dampers, and two negative torques; one which depends on the constants of the interpolar dampers only ($\mathbf{T}_{d_2}$), and one which depends on the constants of both dampers ($\mathbf{T}_{d_3}$); and it is seen that a machine, provided with low-resistance polar dampers only, is more unstable at no load than it would be if the dampers were not fitted.

It is difficult to generalize where so many factors are involved, but the following statements appear to be justified. (1) In machines of low armature resistance and high reactance, the positive damping is nearly independent of this resistance, and the negative damping nearly proportional to it. (2) An increase in reactance in such a machine on no load reduces $\mathbf{T}_{d_3}$ more rapidly than $\mathbf{T}_{d_1}$ and $\mathbf{T}_{d_2}$. (3) In a synchronous motor having large leakage reactance l_a, an increase in excitation causes a smaller increase in $\mathbf{T}_{d_1}$ than in $\mathbf{T}_{d_2}$ and $\mathbf{T}_{d_3}$, since in the first case $I_2(l_a + \kappa)$ must be subtracted from the main flux while in the other two cases $I_2(\kappa - \gamma\lambda)$ is deducted, γ_1 having a maximum value of unity. (4) An increase of load in a machine fitted with dampers will always make for an increase in stability if R_a is small in comparison with $\kappa\omega$ and $\lambda\omega$; for, taking those portions of the torque expression which appear as multiples of the cross flux, we have from Appendix I

$$\mathbf{T}_{d_1} : \mathbf{T}_{d_2} : \mathbf{T}_{d_3} :: \frac{e}{\Omega} : \frac{R_a}{\omega^2} : \frac{R_a^2 c}{\Omega\omega^2(a^2+c^2)}$$

and, moreover, the cross-flux factor in $\mathbf{T}_{d_1}$ is $I_1^2(l_a+\lambda)^2$, while in $\mathbf{T}_{d_2}$ and $\mathbf{T}_{d_3}$ it is $\left[I_1^2(\lambda - \kappa\gamma_2)^2 + (cI_2)^2\right]$, of which the last term may generally be neglected.

Experimental Work.

The experimental confirmation of the expressions obtained for the damping coefficients presents serious difficulties, chiefly owing to the large numbers of factors involved. Thus, although it is comparatively easy to measure the amount of negative damping present in the machine, the estimation of the appropriate values of $(l_a + \kappa)$ and $(l_a + \lambda)$ and of the constants of the damper windings is by no means a simple matter, especially as they are all affected to a greater or less extent by the saturation of the iron. Thus it is difficult to obtain calculated values of the damping which agree closely with the observed magnitudes.

A number of experiments were, however, carried out in the Laboratories of Applied Electricity, University of Liverpool, in an endeavour to obtain [at] any rate a partial confirmation.

First Series.

A 3-phase alternator rigidly connected to a direct-current machine was chosen, the particulars of which are as follows.

A.C. side.—200 (line) volts, 86 amperes, 8 poles, frequency 50 cycles per sec., star-connected; armature resistance, 0·04 ohm; armature turns per pole per phase, 6; damper turns per pole per phase, $\frac{1}{3}$; polar damper resistance, $1\cdot2/10^4$ ohm; interpolar damper resistance, $2/10^3$ ohm; polar and interpolar damper leakage self-inductance (estimated), $4\cdot5/10^7$ henry; armature leakage self-inductance, $8/10^5$ henry.

Demagnetizing coefficient (κ) :—*

Leading current, I_2 =	0	26	30	34 amps.
$\kappa\omega$ =	0·775	0·586	0·555	0·525 ohm

Cross-magnetizing coefficient (λ) :—*

Leading current, I_2 =	0	26	30	34 amps.
$\lambda\omega$ =	0·375	0·285	0·267	0·252 ohm

D.C. side.—460 volts, 80 amperes; armature resistance, 0·185 ohm.

The alternator when directly connected with the city supply, and with the direct-current machine entirely disconnected from the mains, exhibited a very slight instability when excited for unity power factor.

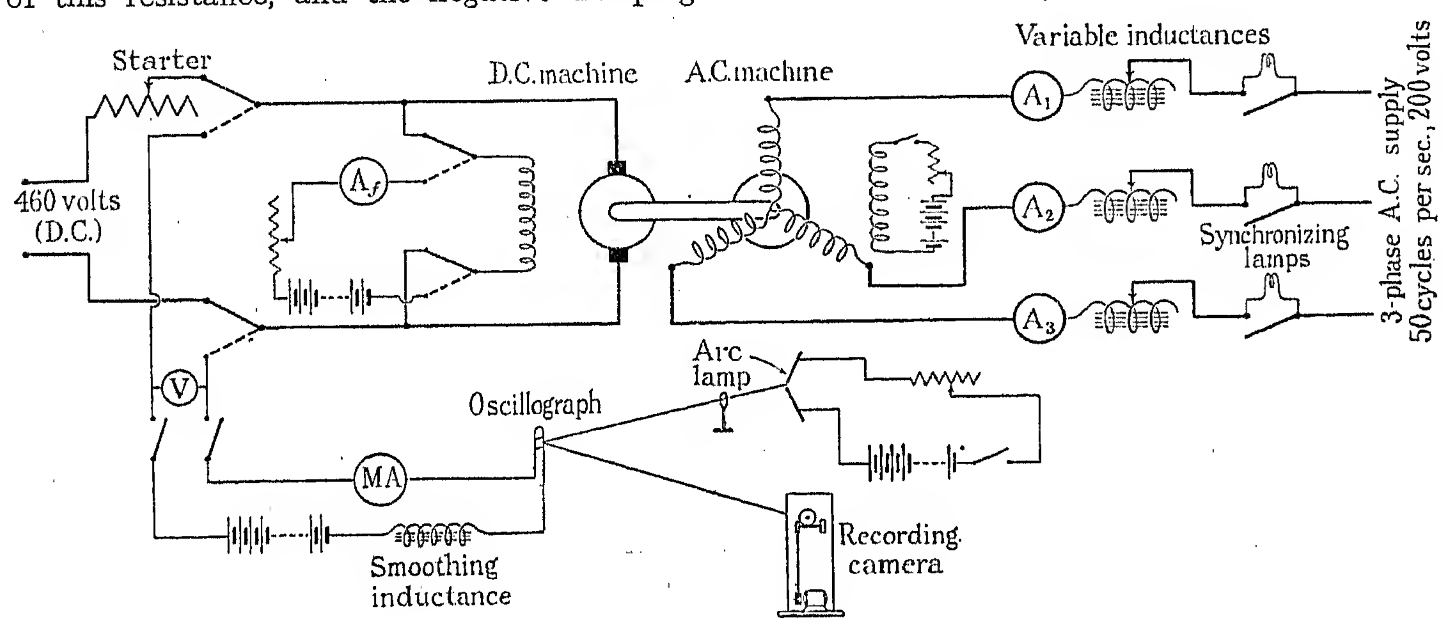

FIG. 2.

* Corresponding to Fig. 4. The variation of κ and λ with wattless current was shown in a paper on "Measurement of the Load Angle of Synchronous Machinery" (*Journal I.E.E.*, 1931, vol. 69, p. 281). The values cited here were measured by the stroboscopic method.

For the purpose of these tests, the connection to the mains on the a.c. side was made through a 3-phase

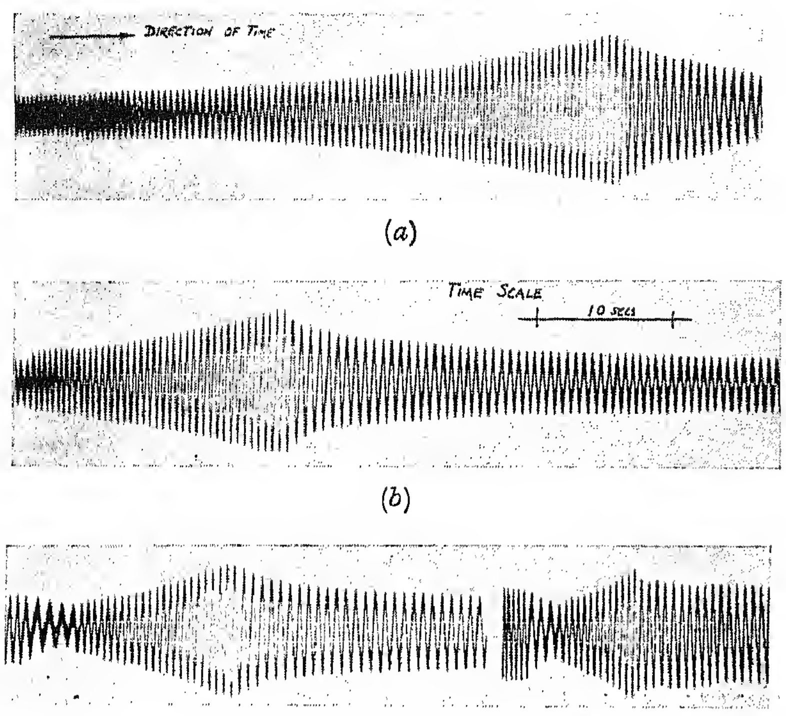

FIG. 3.—Oscillograph records. Field current 11·2 amps.
(a) Core 4 in. out. (b) Core $4\frac{1}{2}$ in. out.
(c) Core 5 in. out. (d) Core $5\frac{1}{2}$ in. out.

choking coil arranged as a series (variable) impedance. The direct-current machine was, after synchronizing, disconnected from the mains, excited by a small field

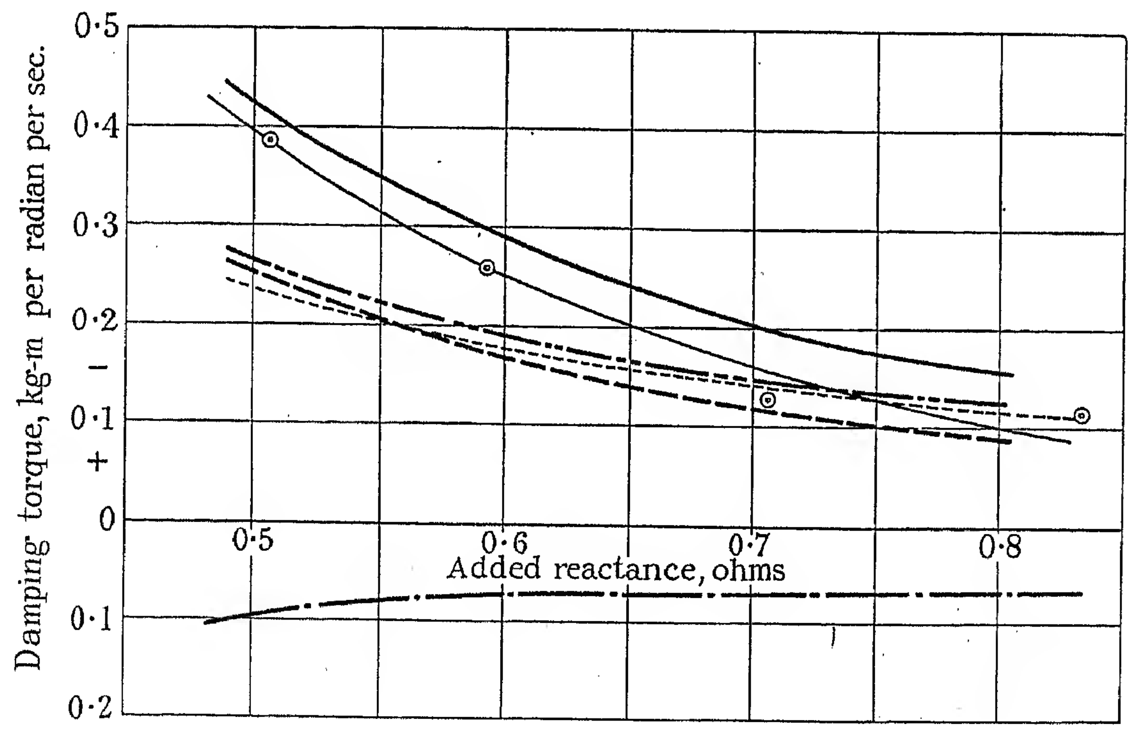

FIG. 4.

Td_1 }
Td_2 } calculated.
Td_3 }
$Td_1 + Td_2 + Td_3$ }
$Td_1 + Td_2 + Td_3$, observed.
Equation (8), calculated.

current, and backed against a battery of cells in series with an oscillograph (Fig. 2).

The field of this machine was adjusted to such a strength that when the alternator was running without oscillation, no current passed through the oscillograph; phase swinging was, however, accompanied by a deflection in the oscillograph, proportional (since the resistance in the armature circuit was large compared with the self-inductance) to the velocity of displacement. Oscillograph records were taken for various values of the series impedance and of the direct-current field of the alternator. An example of these records is shown in Fig. 3.

Measurements of the logarithmic increment* on a number of these curves enabled the value of the damping coefficient to be calculated for various values of field and impedance, and compared with the values obtained from the formulæ. The results for a field current of 11·2 amperes are shown in Table 2 and Fig. 4.

The total damping torque and its components $\mathbf{T}_{d_1}$, $\mathbf{T}_{d_2}$, and $\mathbf{T}_{d_3}$, are plotted for purposes of comparison, and in order to emphasize the importance of the term in R_a^2, $\mathbf{T}_{d_3}$, which in this case makes a large contribution to the total negative damping. The theoretical curve calculated from equation (8) for a machine without dampers is also plotted. The machine with dampers is seen to be more unstable at no load for the lower values of the armature self-inductance than it would be if the damping windings were not fitted.

This damper instability is due to the particular construction of the machine, in which the interpolar damper resistance is 16·7 times the resistance of the dampers encircling the poles. The measurement of the resistance of this interpolar damper presented considerable difficulties, and the value taken for the calculation of the damping coefficient is the average resistance per pole. The value of the leakage reactance of the dampers was calculated from the dimensions. At

* A similar method was used by Cotton (*Journal I.E.E.*, 1931, vol. 69, p. 993).

the maximum value which it is likely to assume, it makes little difference to the calculated value of the torque in this machine.

The following notes on the calculated values may be useful.

$\mathbf{T}_d'$ (*equation* 13).—The term in Φ_0 is the only one of importance. I_1 is small (about 6 amperes), and the second term does not rise to 0·002 kg-m per radian per sec.

$\mathbf{T}_{d_2}$ (*equation* 18, *Appendix I*).—The terms in ce and those in Φ_2 and I_1 are negligibly small.

$\mathbf{T}_{d_3}$ (*equation* 19, *Appendix I*).—All the terms may be neglected except

$$-\frac{1}{2X_0^2\Omega}(\Phi_1 + aI_2)^2\frac{R_a^2 c}{\omega^2}$$

The values of β_1/Ω, β_2/Ω, γ_1, and γ_2, are all affected to a certain extent by the frequency of swing (which decreases as reactance is introduced into the armature circuit), though γ_1 is practically constant in this machine, owing to the high interpolar resistance.

To give an idea of the magnitudes, the limiting values are shown in Table 1.

TABLE 1.

	Added reactance	Ω	β_1/Ω	β_2/Ω	γ_1	γ_2
At 22 amperes leading current ..	0·5 ohm	17·0	$1{\cdot}23/10^3$	$2{\cdot}63/10^2$	1·0	0·646
At 34 amperes leading current ..	0·83 ohm	14·1	$1{\cdot}44/10^3$	$3{\cdot}34/10^2$	1·0	0·664

Second Series.

In this series the connections on the alternating-current side were maintained as in the first series, but the direct-current side was made to act as a damper of adjustable magnitude* by eliminating the oscillograph and completing the circuit through a variable resistance.

Instability having been introduced on the a.c. side by a suitable (measured) adjustment of field or self-inductance, the armature resistance and field on the d.c. side were adjusted till a slight constant oscillation was just maintained. Under these circumstances the negative damping on the a.c. side could be equated to the damping inherent in the machines, plus the damping effect of the currents in the d.c. armature, which latter is susceptible of fairly exact computation.

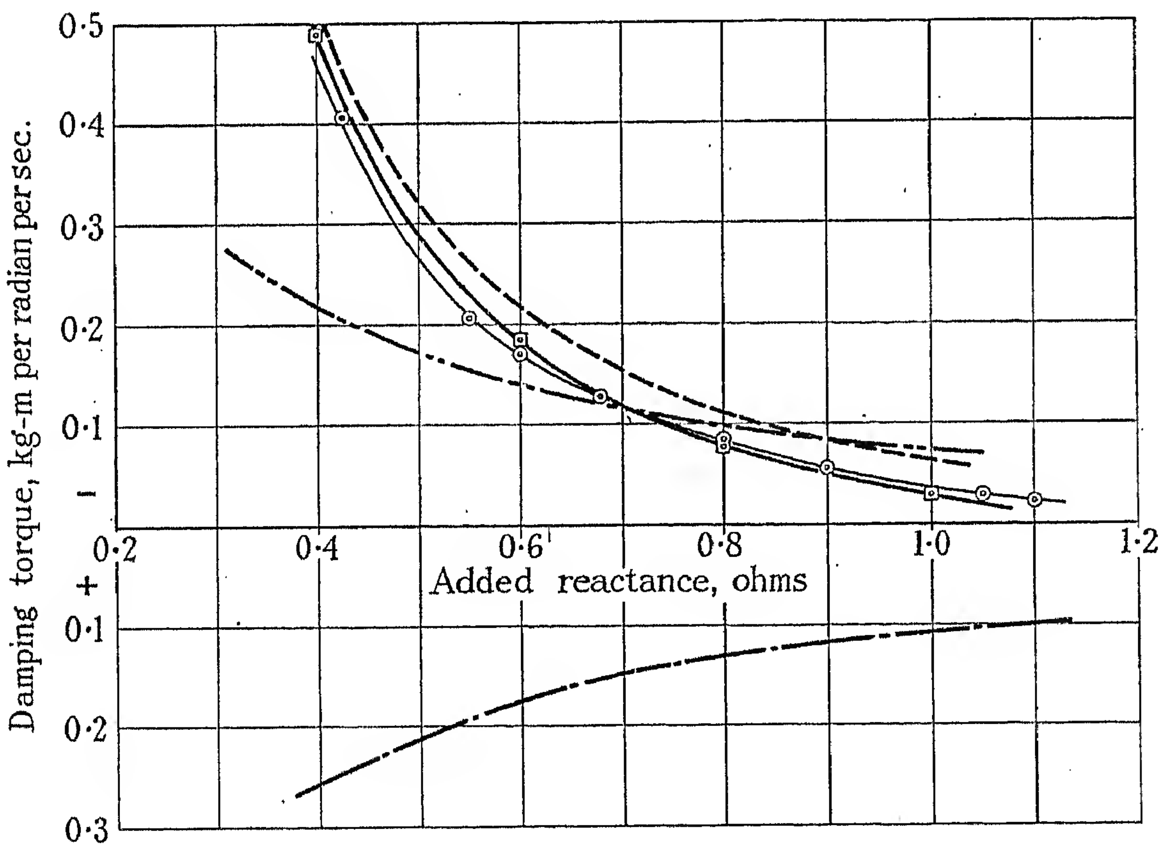

FIG. 5.

$T_{d_1} + T_{d_2} + T_{d_3}$ observed.
$T_{d_1} + T_{d_2} + T_{d_3}$ } calculated.
T_{d_1}
T_{d_2}
T_{d_3}

In this way it was possible to obtain a check on the results in the first series, and also to investigate the

* See Appendix III.

effects of load on the negative damping coefficient, though here a limitation was experienced in that the damping due to the direct currents became so high on the larger loads (owing to the inevitable decrease of armature resistance and increase of field) that it was impossible to render the set unstable.

The results are plotted in Fig. 5. The a.c. machine, operating as a motor, is taking a load of 3 kW to supply its own losses and those of the d.c. machine to which it is connected. The component torques $\mathbf{T}_{d_1}$, $\mathbf{T}_{d_2}$, and $\mathbf{T}_{d_3}$, are plotted again here as in Fig. 4. The load current, now 8·7 amperes, contributed a positive torque of about 0·03 kg-m per radian per sec. to $\mathbf{T}_{d_1}$. As before, at low values of the added inductance $\mathbf{T}_{d_3}$ afforded most of the negative damping. An abstract of the calculations for Fig. 5 is given below.

Production of Unstable Conditions.

As remarked on page 500, any alternator of normal design has a tendency towards instability in virtue of the fact that the armature reactance exceeds the armature resistance, but as a rule this excess is so large as to make the negative damping less than the positive damping inherent in the machine (due to the eddy-current and damper torques, windage, and the like). If, however, the negative damping be increased by the introduction of resistance into the armature circuit, the motion will in many ways become unstable, especially in small machines* on light load.

For example, an alternator having the following particulars was found to be perfectly stable when excited for unity power factor on the city mains at no load: line voltage, 200 volts; line current, 58 amperes; resistance per phase, 0·1 ohm (average); reactance per phase, 1·36 ohms† (average); speed, 1 200 r.p.m.; frequency, 50 cycles per sec.

When the armature resistance was increased the machine began to be unstable, and exhibited maximum instability when the total resistance in the armature circuit was 0·56 ohm per phase, giving a ratio of $R_a/(L_a\omega) = 0\cdot408$ as against the theoretical value of 0·415. With this value of the armature resistance it was impossible to operate the machine at all on no load unless it was connected with a d.c. machine which could supply a positive damping torque (see Appendix III).

The machine had no dampers in the interpolar space, and the damper encircling the polar axis was represented by the field winding and the eddy-current paths in the pole faces. It thus approximated to a machine without dampers, to which equation (4*a*), and, more rigorously, equation (8), apply.

* In which it is the general practice to omit the interpolar dampers and to fit only dampers surrounding the poles.

† Obtained from a measurement of synchronous impedance.

Abstract of Experimental Results.

First Series.

The values of the damping coefficient given in Table 2 were obtained from a series of oscillograms (Fig. 3) taken with $E = 242$ volts, and were calculated from the following relations.

The equation of motion is

$$\mathbf{I}d^2\theta/dt^2 + bd\theta/dt + c\theta = 0$$

whence $\theta = \epsilon^{-\lambda' t}\cos(\psi t + \xi)$

where $2\lambda' = b/I,\ \gamma'^2 = c/I,$ and $\psi^2 = (\gamma'^2 - \lambda'^2)$.

Table 2.

Damping coefficient from above relations. A.C. voltage (E) = 242 volts.
Constant damping torque due to a.c. field = 0·02 kg-m.

Mean decrement (σ')	0·946	0·941	0·879	0·833
$\log_e \sigma'$	−0·056	−0·061	−0·129	−0·185
Periodic time (T'), in sec.	1·335	1·275	1·2	1·11
Damping coefficient, in kg-m per radian per sec.	−0·093	−0·106	−0·239	−0·369
Added reactance, in ohms	0·833	0·707	0·594	0·507

The decrement of the curve is given by

$$\sigma = \epsilon^{\frac{\lambda'\pi}{\psi}} = \epsilon^{\frac{bT}{4I}} = (\theta_1 + \theta_2)/(\theta_2 + \theta_3)$$

where T = periodic time = $2\pi/\psi$.

Hence $$b = \frac{4I}{T}\log_e\frac{(\theta_1 + \theta_2)}{(\theta_2 + \theta_3)}$$

As the values of the successive swings θ_1, θ_2, etc., are so close together, giving a small difference in amplitude, θ_1, θ_7, and θ_{13} were used, giving $b = [2\mathbf{I}/(gT')]\log_e\sigma'$ (in kg-m per radian per sec.), where T' = time for 3 swings and σ' = decrement $= (\theta_1 + \theta_7)/(\theta_7 + \theta_{13})$.

Second Series.

Estimation of Total Positive Damping.—The damping due to the eddy currents in the a.c. and d.c. machines

was determined from a series of Kapp lines taken at various values of the a.c. and d.c. field currents.

The damping due to the d.c. armature current may be calculated from equation 24 (Appendix III), which gives

$$\mathbf{T}_{d_{DC}} = E^2/(\overline{\omega}^2 R_T \times 9\cdot 81) \text{ kg-m per radian per sec.}$$
$$= 0\cdot 0000165 E^2/R_T$$

where E = d.c. electromotive force, R_T = total resistance in the armature circuit, and $\overline{\omega} = 2\pi n'$, n' being the speed of rotation in revolutions per sec.

For instance, taking the point in Fig. 5 corresponding to a choking-coil reactance of 0·6 ohm, the following quantities were observed:—

Armature e.m.f. (d.c.) = 476 volts.
Resistance in d.c. armature circuit = 29·08 ohms.
Field current (d.c.) = 0·97 ampere.
Field current (a.c.) = 6·3 amperes.

Hence

Damping due to d.c. armature current	=0·129	kg-m
Damping due to d.c. field (from Kapp test)	=0·0265	kg-m
Damping due to a.c. field (from Kapp test)	=0·0165	kg-m
Total damping	=0·172	kg-m

where
$$P = [\Phi_0 - (\kappa - \lambda\gamma_1)I_2 + \kappa\beta_2 I_1]\theta_0,$$
$$P' = [\Phi_0 - (\kappa - \lambda\gamma_1)I_2 - \kappa\beta_2 I_1]\theta_0,$$
$$Q = [(\lambda - \kappa\gamma_2)I_1 + \lambda\beta_1 I_2]\theta_0,$$
$$Q' = [(\lambda - \kappa\gamma_2)I_1 - \lambda\beta_1 I_2]\theta_0,$$
$$s = (\omega + \Omega), \quad \text{and} \quad d = (\omega - \Omega).$$

These can be solved fairly simply by writing

$$R_1 = (R_a/s) + c, \qquad R_2 = (R_a/d) - c,$$
$$R_3 = (R_a/s) + e, \quad \text{and} \quad R_4 = (R_a/d) - e.$$

When the values of A, A', B, and B' have been obtained in terms of the constants, we may write, since ω is large compared with Ω,

$$R_1 = \left(\frac{R_a}{\omega} - \frac{R_a\Omega}{\omega^2} + c\right), \quad R_2 = \left(\frac{R_a}{\omega} + \frac{R_a\Omega}{\omega^2} - c\right), \text{ etc.}$$

Substituting the values of A, A', B, and B', in the torque equation, we obtain the terms in $R_a\Omega$ and R_a^2 as follows:

$R_a\Omega$ terms.

$$\mathbf{T}_{d_2} = -\frac{R_a}{2X_0^2\omega^2}\Big\{\left[(\Phi_1 + aI_2)^2 + (cI_2)^2\right]\left[(b^2 + e^2) - \frac{R_a^2}{\omega^2}\right] + 2ce\left(\Phi_1^2 - \frac{R_a^2 I_2^2}{\omega^2}\right)$$
$$+ \left[(\Phi_2 - bI_1)^2 + (eI_1)^2\right]\left[(a^2 + c^2) - \frac{R_a^2}{\omega^2}\right] - 2ce\left(\Phi_2^2 + \frac{R_a^2 I_1^2}{\omega^2}\right)\Big\} \quad . \quad (18)$$

R_a^2 terms.

$$\mathbf{T}_{d_3} = -\frac{1}{2X_0^2\Omega}\Big\{\left[(\Phi_1 + aI_2)^2 + (cI_2)^2\right]\frac{R_a^2 e}{\omega^2} + \frac{2R_a^2 bc}{\omega^2}\Phi_1 I_2 - \frac{R_a^4 cI_2^2}{\omega^4}$$
$$+ \left[(\Phi_2 - bI_1)^2 + (eI_1)^2\right]\frac{R_a^2 c}{\omega^2} - \frac{2R_a^2 ae}{\omega^2}\Phi_2 I_1 - \frac{R_a^4}{\omega^4}eI_1^2\Big\} \quad . \quad (19)$$

Conclusion.

The authors wish to express their thanks to Prof. E. W. Marchant for putting at their disposal instruments and apparatus in the Laboratories of Applied Electricity, University of Liverpool, and for his help and encouragement; to Mr. U. S. Haslam-Jones, M.A., for his criticism of the mathematical analysis; and to those honours students in the University who assisted in the production of the experimental data.

APPENDIX I.

To solve the equation

$$\frac{d(\delta\Phi)}{dt} + l_a\frac{d(\delta i)}{dt} + R_a\delta i = 0$$

we first introduce the values of $\delta\Phi$ and δi, and obtain, by equation of coefficients, the following:—

$$A(R_a + cs) + B'(R_a + es) - A'as + Bbs - Ps = 0$$
$$A(R_a - cd) + B'(R_a - ed) - A'ad + Bbd - P'd = 0,$$
$$A'(R_a + cs) - B(R_a + es) + Aas + B'bs + Qs = 0,$$
$$A'(R_a - cd) + B(R_a - ed) - Aad + B'bd + Q'd = 0,$$

where $\Phi_2 = (l_a + \lambda)I_1$ and $\Phi_1 = \Phi_0 - (l_a + \kappa)I_2$.

Selecting the terms in $(\Phi_1 + aI_2)$ as being large compared with all other terms, we obtain the values given on pages 504 and 505.

Both these torques must be multiplied by the factor np^2/g for a machine of n phases and p pole pairs.

APPENDIX II.

Inherent Positive Damping.

As a postscript to the treatment of the negative damping coefficients, it is not out of place to discuss briefly the chief sources of positive damping which occur in synchronous machines and in machinery which is frequently connected to them.

The iron losses in an alternator may be written

$$W_L = c_1 B^{1\cdot 6} f + c_2 B^2 f^2$$

where B is the flux density in the iron, f is the frequency of supply, and c_1 and c_2 are constants.

The torque due to the iron loss may thus be written

$$\mathbf{T}_L = c_1' B^{1\cdot 6} + c_2' B^2 \overline{\omega}$$

where $\overline{\omega}$ is the angular speed of rotation and c_1' and c_2' are constants. Thus the second component of the torque, i.e. that due to eddy currents, is proportional to the velocity and acts as a true viscous damping.

If x be the percentage of the total output required to overcome the losses in the machine, and if $\frac{1}{2}x$ may be taken as the percentage due to the losses in the iron, then, if one half of this be due to eddy loss, we have

$$\text{Eddy loss} = \tfrac{1}{4}xEI_{FL},$$
$$\text{Torque due to eddy loss} = \tfrac{1}{4}x\mathbf{T}_{FL},$$

and, since this torque is proportional to the angular velocity,

$$\mathbf{T}_d = \tfrac{1}{4}x\mathbf{T}_{FL}/\overline{\omega},$$

where $\mathbf{T}_{FL}$ is the full-load torque and $\overline{\omega}$ the angular velocity at which this torque is measured.

APPENDIX III.

The Damping Action of a Direct-Current Armature (Without Interpoles).

Consider a compound-wound machine (Fig. 6), shunt-excited, working against a load of back-e.m.f. e, resistance R_x, and self-inductance L_x. Taking L_a, L_f, and L_s to be the self-inductances of the armature, the shunt

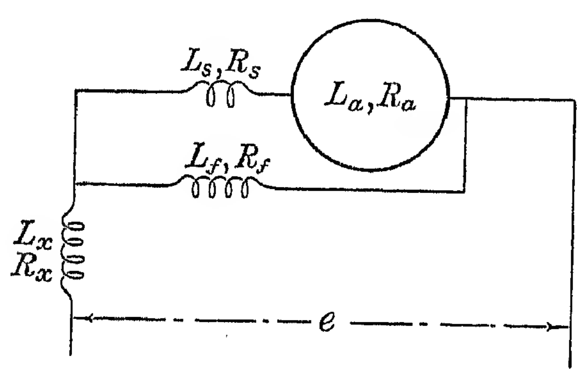

Fig. 6.

winding, and the series winding respectively, and R_a, R_f, and R_s to be their respective resistances, we have

$$e_a - iR_T - L_T di/dt - M di_f/dt = e \quad . \quad . \quad . \quad . \quad . \quad (20)$$

$$e_a - iR - (L+M)di/dt - (L_f+M)di_f/dt - i_fR_f = 0 \quad . \quad (21)$$

where i = armature current, i_f = shunt field current (neglected in comparison with i), e_a = armature e.m.f., M = coefficient of mutual induction between series and shunt fields, $R_T = R + R_x$, $L_T = L + L_x$, $R = R_a + R_s$, and $L = L_a + L_s$. Hence

$$L_f di_f/dt + R_f i_f - R_x i + (M - L_x)di/dt = e \quad . \quad (22)$$

Now let the armature oscillate about its steady angular velocity so that

$$e_a' = e_a - e_a'' \sin \Omega t$$
$$i = I_1 \sin \Omega t + I_2 \cos \Omega t + I_0$$
$$i_f = i_1 \sin \Omega t + i_2 \cos \Omega t + i_0$$

For the oscillating terms, equation (22) becomes

$$L_f di_f/dt + R_f i_f - R_x i + (M - L_x)di/dt = 0$$

and hence, equating coefficients of sine and cosine, we obtain $i_1 = aI_1 + bI_2$, and $i_2 = -bI_1 + aI_2$,

where $\quad a = [R_xR_f - L_f(M - L_x)\Omega^2]/z,$

$\quad b = [R_xL_f\Omega + R_f(M - L_x)\Omega]/z,$

and $\quad z = R_f^2 + L_f^2\Omega^2.$

The armature e.m.f. may be written

$$e_a = \overline{\omega}[c' + \kappa(N_1 i_f + N_2 i)]N_a$$

where $\overline{\omega}$ is the angular velocity of the armature, N_1 and N_2 are the numbers of turns on the shunt and series fields respectively, $\kappa = \tan \theta$, and c' has the value exhibited on the saturation curve in Fig. 7, where the gap flux Φg is measured in hundreds of megalines per radian.

When oscillation occurs, $\overline{\omega}$ becomes $(\overline{\omega} - \theta_0\Omega \sin \Omega t)$, and e_a becomes

$$e_a' = N_a(\overline{\omega} - \theta_0\Omega \sin \Omega t)\{c' + \kappa[N_1(i_1 \sin \Omega t + i_2 \cos \Omega t + i_0) + N_2(I_1 \sin \Omega t + I_2 \cos \Omega t + I_0)]\} \quad (23)$$

where N_a is the number of armature turns in series between brushes.

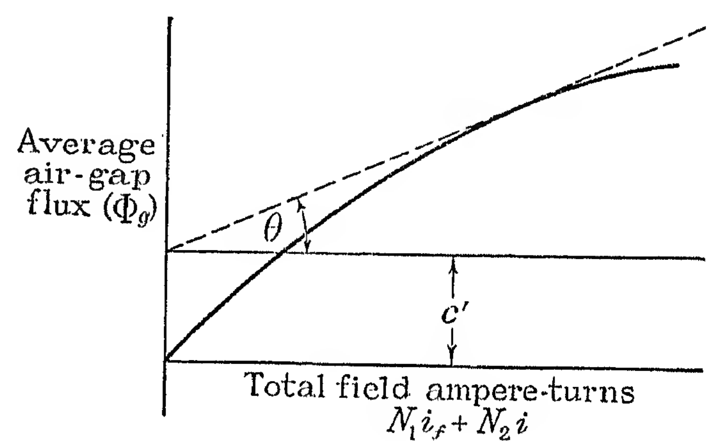

Fig. 7.

Neglecting the terms in the products of $\sin \Omega t$ and $\cos \Omega t$, we have the oscillating terms $e_a' - e_a = \delta e_a$ where

$$\delta e_a = N_a\{\overline{\omega}\kappa[N_1(i_1 \sin \Omega t + i_2 \cos \Omega t) + N_2(I_1 \sin \Omega t + I_2 \cos \Omega t)] - \theta_0\Omega e_a \sin \Omega t\}$$

For the oscillating terms, equation (20) becomes

$$\delta e_a = R_T(I_1 \sin \Omega t + I_2 \cos \Omega t) + L_T\Omega(I_1 \cos \Omega t - I_2 \sin \Omega t) + M\Omega(i_1 \cos \Omega t - i_2 \sin \Omega t)$$

Equation of coefficients gives

$$I_1 = \theta_0 c e_a \Omega/(c^2 + d^2) \quad \text{and} \quad I_2 = \theta_0 d e_a \Omega/(c^2 + d^2),$$

where $\quad c = \overline{\omega}\kappa N_a(N_1 a + N_2) - (M\Omega b + R_T)$

and $\quad d = \overline{\omega}\kappa N_1 N_a b + (L_T + M_a)\Omega$

The average flux per radian in the air-gap is

$$\Phi_g = c' + \kappa(N_1 i_f + N_2 i)$$

whence the average gap density is

$$B_g = \left[c' + \kappa(N_1 i_f + N_2 i)\right]/(lr)$$

where l is the effective length, and r the effective radius, of the armature in cm.

The torque is

$$\begin{aligned} &N_a B_g l r i/10 \\ &\quad = N_a\left[c' + \kappa(N_1 i_f + N_2 i)\right] i \times 10^8/(10 \times 981) \text{ g-cm} \\ &\quad = N_a\left[c' + \kappa(N_1 i_f + N_2 i)\right] i/9{\cdot}81 \text{ kg-m}\end{aligned}$$

Substituting for i and i_f, and selecting the terms containing $\sin \Omega t$, we obtain

$$\mathbf{T}_d' = \frac{\theta_0 \Omega e_a N_a}{(c^2 + d^2)g}\left\{c\Phi + I_0\left[c(N_1 a + N_2)\kappa + N_1 \kappa b d\right]\right\}$$

Substituting here for a, b, c, and d would produce a very complicated expression. We may, however, make some simplifying assumptions. Considering a and b, M is much greater than L_x, M is much less than L_f, and R_x than R_f. Thus a and b are both small (less than $0{\cdot}001$) in machines where R_f is small compared with $L_f\Omega$, and where $L_f\Omega$ itself is large (5 000 ohms, say). We may therefore write

$$c = \varpi \kappa N_2 N_a - R_T$$

while d^2 may be neglected in comparison with c^2. The term in I_0 may also be neglected in comparison with $c\Phi$. Thus,

$$\begin{aligned}\mathbf{T}_d' &= \theta_0 e_a \Phi N_a \Omega/\left[(\varpi\kappa N_2 N_a - R_T)g\right] \\ &= \theta_0 e_a^2 \Omega/\left[\varpi(\varpi\kappa N_2 N_a - R_T)g\right]\end{aligned}$$

$$\mathbf{T}_d = \frac{e_a^2}{\varpi(\varpi\kappa N_2 N_a - R_T)g}$$

the unit in both cases being kg-m per radian per sec.

For a separately excited machine (uncompounded) N_2 is zero, and the torque for unit angular velocity is given by

$$\mathbf{T}_d = e_a^2/(\varpi R_T g) \quad . \quad . \quad . \quad . \quad . \quad (24)$$

In the foregoing discussion the explicit formula for the damping torque has been modified by reference to a machine of a particular specification, but it must be remembered that the same modifications are not universally applicable.

Returning to the expression for the torque of a separately excited machine we have, if x be the total loss (iron and copper) expressed as a percentage of the full-load output, and $\frac{1}{2}x$ the percentage copper loss,

$$R_a I_{FL}^2 = \tfrac{1}{2}x E I_{FL}$$

where I_{FL} = full-load current* and E = terminal e.m.f.

Thus $\qquad R_a = \tfrac{1}{2}xE/I_{FL}$

and $\qquad \mathbf{T}_d = 2EI_{FL}/(x\varpi)$

$\qquad\qquad = 2\mathbf{T}_{FL}/(x\varpi)$

where $\mathbf{T}_{FL}$ = torque at full load.

* Assuming for simplicity that the external resistance of the armature circuit is small compared with the resistance in the machine itself.

APPENDIX IV.

Positive Damping of a Steam Turbine.

An alternator rigidly connected to a steam turbine experiences a damping torque whose magnitude is proportional to the load on the turbine. Three types of turbines may be considered.

(i) *Impulse Type.*

If v_1 be the initial velocity of the steam leaving the nozzle, and u the linear velocity of the blades, the load torque is given by

$$\mathbf{T}_{ql} = (v_1 \cos \alpha - u)(1 + c)(m/g)\rho$$

where α is the angle between the direction of the steam jet and the direction of motion of the blades, ρ the effective radius of the ring of blades, $c = \cos\theta_2/\cos\theta_1$, where θ_1 and θ_2 are respectively the inlet and outlet angles of the blades (this ratio may be considered to be independent of blade velocity), and m = mass of steam flowing per sec.

The turbine works with maximum efficiency for a blade speed such that $v_1 \cos \alpha = 2u$. Thus the torque on load is given by

$$\mathbf{T}_{ql} = u(1 + c)(m/g)\rho \quad . \quad . \quad . \quad . \quad (25)$$

The effect of a small increment of blade velocity δu will cause a change in torque (equation 25) given by

$$\begin{aligned}\delta\mathbf{T}_{pl} &= -\,\delta u(1 + c)(m/g)\rho \\ &= (\delta u/u)\mathbf{T}_{ql}\end{aligned}$$

The damping torque (due to the steam) for unit angular velocity is therefore given by $\mathbf{T}_{ds} = \mathbf{T}_{ql}/\varpi$, where ϖ is is the angular velocity of rotation.

(ii) *Impulse Turbine Compounded for Velocity.*

In this case the load torque for n rows of moving blades is given by

$$\mathbf{T}_{ql} = 2n(v_1 \cos \alpha - nu)(m/g)\rho$$

when $\cos\theta_1 = \cos\theta_2$.

For maximum efficiency, $u = v_1 \cos \alpha/(2n)$.

Thus $\mathbf{T}_{ql} = 2n^2\omega m\rho/g$, and the damping torque due to the steam for unit angular velocity is given by $\mathbf{T}_{ds} = \mathbf{T}_{ql}/\varpi$.

(iii) *Reaction Turbine.*

Here the load torque is given, to a good degree of approximation, by

$$\mathbf{T}_{ql} = n(2v_1 \cos \alpha - u)(m/g)\rho$$

where n is the number of stages.

For maximum efficiency, $u = v_1 \cos \alpha$.

Then $\mathbf{T}_{ql} = um\rho/g$, and the damping torque due to the steam for unit angular velocity is given by $\mathbf{T}_{ds} = \mathbf{T}_{ql}/\varpi$.

Thus, in each of the three cases, the damping is directly proportional to the load on the turbine (since it is proportional to m) and inversely proportional to the angular velocity for most efficient operation. Any given turbo-alternator will therefore become more stable as the load increases, not only on account of the increased electromagnetic damping due to any damping grids that may be fitted but also on account of an increase in the damping afforded by the turbine itself.

RADIATION AND ELECTRICAL POWER TRANSMISSION.*

By W. E. Sumpner, D.Sc., Member.

(*Paper first received* 2nd *September*, 1933, *and in final form* 15th *March*, 1934.)

Summary.

The paper discusses the mode of propagation of energy under steady current conditions, and advances the view that it is only a special case of radio transmission. The electromagnetic theory governs all electrical processes, but, while the high-frequency disturbances of light, or radio-telephony, move in waves which are known to retain their individuality when superposed, the principle of independence does not seem to be applied when the disturbances are of very low frequency so as to approach steady current conditions. In the last case the electromagnetic fluxes are, even nowadays, often regarded as static, and when such fluxes are superposed it is generally assumed that they merge into a single flux. Poynting's theorem of energy flow suggested that the fluxes due to steady currents are moving through the dielectric, but tacitly assumed that the fluxes form a single stream, and that nothing of the nature of reflection occurs when this stream reaches a material surface.

It would seem only to harmonize with Maxwell's theory, and with the properties of light, if such streams when incident on matter were to give rise to reflected ones, and therefore to a system of superposed streams. The aim of the paper is to show that such a view is quite consistent with the known distribution of energy to the various parts of the conducting circuit.

The mathematical problem is discussed in Part 2 of the paper. The analysis is an example of Heaviside's vector methods. It is necessarily based on assumptions. The physical aspects and the justification of these assumptions are dealt with in Part 1.

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PART 1.

(1) Introduction.

The aim of the present paper is to justify the view that the processes at work in steady current systems of electrical power transmission are the same as those involved in radiation, and that the fluxes have the same characteristics as those in a wave of light. This is not the normal view. Maxwell's theory is an electromagnetic theory of light, but does not seem to have been used as an optical theory of electromagnetic processes. No one seems to have applied to it two principles always admitted about light: (i) the individuality and independence of light waves, and (ii) the production of reflections when light waves are incident on matter.

It seems natural to expect the flux streams due to a steady current supply system to behave in general like those of radiation. What is hard to see is that such a view is consistent with the known facts about the distribution of energy in electrical circuits.

The actual mode of transfer of energy through the ether is quite as important to the practician as to the theorist. The engineer deals directly only with matter, but the object of his work is to cause and to control the movement of energy of the type he needs from the place where he can get it to the place where he can use it. The flow of energy in steady current circuits is specified by the Poynting flux. In order to illustrate this flux, let us suppose that a battery of cells (G, Fig. 1) supplies a current of 10 amperes at 100 volts to a load L through mains of negligible resistance. These mains denote electrical level surfaces, one at 100 volts, the other at zero volts. Their path may be of any shape. In order to suggest the complications which readily arise in actual circuits a zigzag course has been chosen for one of the mains. Apart from a few very special cases, nothing precise is known about the position of any level surface at an intermediate voltage, say 90 volts, except that it must join the 90-volt points on the battery to the 90-volt points on the load, and even these points are quite arbitrary so far as the diagram is concerned. Each level surface must completely enclose one of the two mains. The sections, by the plane of the paper, of the surfaces at 90 and 60 volts are approximately as shown by the dotted lines, while the arrows indicate the direction of energy flow. Poynting's theorem tells us that energy, at a rate defined by 30 volts and 10 amperes, passes from the battery into the dielectric between these surfaces, flows like a river between banks formed by them, and finally enters the load. It is supposed that the level surfaces indicate the true streamlines, and that the load acts like an ocean—absorbing the whole river without returning a drop. Radiant energy when incident on a material surface is always in part reflected. A perfect conductor, according to Maxwell's theory, is impervious to electromagnetic fluxes. The load L may consist for the most part of the nearly perfect conductor, copper,

* The Papers Committee invite written communications, for consideration with a view to publication, on papers published in the *Journal* without being read at a meeting. Communications (except those from abroad) should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

yet the Poynting flux of energy is supposed to be completely absorbed on incidence. Such results suggest that the Poynting flux may indicate a net effect denoting the excess of the incident over the reflected energy.

The level surfaces are rigidly fixed at G and at L, but seem to be wonderfully flexible in the dielectric. They can be altered by shifting the mains, or by using a metal plate connected by a wire to some point on the circuit, or by influences due to some quite distinct circuit through which a steady current is flowing. In no case is any change produced in the amount of energy supplied by the generator to a particular part of the load.

Poynting's theorem was no sooner published than it was shown by Sir J. J. Thomson that the energy flux **P** is not the only mathematical solution possible, and that a flux **R** will equally well satisfy all the conditions provided that **T**, or (**P** − **R**), the difference between **P** and **R**, is what is known as a circuital flux.

On page 313 of "Recent Researches in Electricity and Magnetism," published in 1893, Thomson states, in

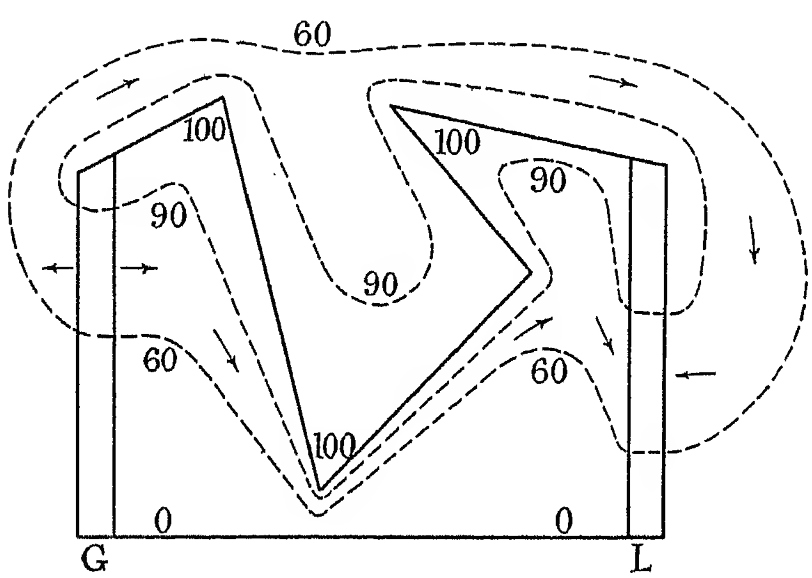

FIG. 1.

reference to the proof of the Poynting flux **P**, that "it does not however justify us in asserting that the flow of energy at any point *must* be that given by" **P**, since "we can find an indefinite number of quantities" **T**, "of the dimensions of flow of energy, which satisfy the condition" $\iint \mathbf{T}ds = 0$, "where the integration is extended over any closed surface." No one seems to have made any use of this flux. Heaviside regarded it as needless. He said that it represents energy that simply goes round and "does nothing," and that "being useless it would be superfluous to bring it in." The question at issue, however, is whether the true flux is **P** or **R**. If several solutions are possible **P** may be the one most convenient mathematically while **R** may be the one which fits in best with the physical facts.

A circuital flux is simply an eddy. Poynting overlooked the possibility of eddies. These can exist however steady the conditions, and can be quite as steady as the main stream itself. An eddy may be placed between two level surfaces V_1, V_2, as at T_1 in Fig. 2, or may cross one of these surfaces as at T_2. In the latter case the energy crossing the surface at one spot must recross that surface at another. In neither case does the eddy affect the net flow of energy from G to L between the two equipotential surfaces V_1 and V_2. The eddy is not necessarily small. It may, like T_3, extend from generator G to load L. The net flow of energy determined by **P** is not inconsistent with the coexistence with **P** of additional fluxes of energy passing to and fro between generator and load. Moreover, the eddy T_3, though it must be continuous, need not follow a smoothly curved path. It may zigzag from surface to surface of the matter in a way suggestive of reflected rays, and of rays that are reflected perfectly, since it can be assumed that all energy actually absorbed by the matter is supplied by the flux **P**.

The discharge of a stream into a lake S need not be altered by changing the distribution of its flow from a current density given by **P** to another given by **R**. If **T**, or (**P** − **R**), is a circuital flux, any portion of **T** entering S at one place must leave it at another, so that we have

$$\iint \mathbf{T}ds = 0, \quad \text{or} \quad \iint \mathbf{P}ds = \iint \mathbf{R}ds$$

where each integral is taken over the boundary of the lake. The same result will follow if S, instead of being

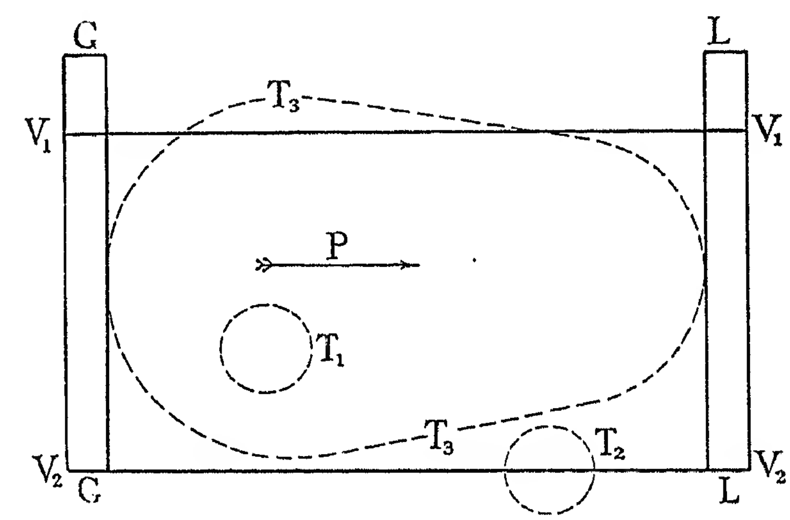

FIG. 2.

the shore of a lake, is a closed surface completely surrounding a volume.

Now if **R** consists of a number of superposed fluxes of radiant energy,

$$\mathbf{R} = \mathbf{R}_1 + \mathbf{R}_2 + \ldots$$

each having the characteristics of a beam of light, each of these superposed fluxes will be the Poynting flux for the corresponding beam, and can be obtained by using the Poynting formula in conjunction with the electric and magnetic forces of that beam. We can obtain a compound Poynting flux **P** by adding vectorially the electric forces of all the radiant fluxes to form a resultant electric force, by adding similarly the magnetic forces to form a resultant magnetic force, and finally by using these resultant forces with the Poynting formula.

The compound flux **P** will not be the same as **R**, but, if it can be shown that the difference is always a circuital flux, we must have

$$\iint \mathbf{P}ds = \iint (\mathbf{R}_1 + \mathbf{R}_2 + \ldots)ds$$

for any completely closed surface. This will mean that the energy absorbed within, or emitted from, the volume enclosed by the surface S can be calculated either from

the flux **P** or from the flux **R**, and will therefore show that there is nothing inconsistent with the known distribution of energy in the circuit, if the Poynting flux **P**, in conjunction with **T**, is interpreted physically as resulting from a number of superposed radiant fluxes $\mathbf{R}_1$, $\mathbf{R}_2$, etc. Such a result will in no way lessen the importance and convenience of the Poynting flux, but will make this flux appear more wonderful than ever. If the actual facts correspond with a superposed system of fluxes it is a most extraordinary result that a single continuous flux can be found to represent the net flow of energy from one part of the circuit to another.

In order to justify the foregoing interpretation it appears that there are three distinct problems to be solved. Two of these are entirely physical, while the third is entirely mathematical. They are:—

(*a*) To justify on physical grounds a radiation assumption that the electromagnetic fluxes associated with a steady current circuit consist of a system of superposed radiation fluxes $\mathbf{R} = \mathbf{R}_1 + \mathbf{R}_2 + \ldots$, from which the Poynting flux **P** can be found in the ordinary mathematical way by vector summation of like forces.

(*b*) To justify on physical grounds a local-impulse assumption used in the mathematical argument whenever there is a transformation of energy from one state to another, one of these states corresponding with the electromagnetic fluxes in the dielectric, and the other being the energy state of the matter.

(*c*) To show mathematically, on the basis of these assumptions, that the difference between **P** and **R** is always a circuital flux.

The paper thus naturally consists of two parts. One of these deals with the physical assumptions, and the other deals with the analysis. The Appendix contains a further discussion of the local-impulse assumption.

(2) Wave Motion and Direct-Current Circuits.

The idea that waves occur when an electromotive force is suddenly applied to an electrical circuit is by no means a new one in the mathematical treatment of the subject. Heaviside, when dealing with his cable problem, constantly alluded to such waves. He always treated them as if they were waves of light and subject to reflections. His treatment of these waves mathematically, however, was markedly different from that of any other writer. He showed interest only in the front of the advancing disturbance, and paid no attention to the rest of the wave. Everything he did was consistent with the view that he was studying the action of a thin pulse of light, including that of the reflections resulting from its incidence on bounding surfaces. For this purpose he assumed the existence of a "unit function" $H(x)$ whose value is zero for all negative, and unity for all positive, values of x. In terms of this function he defined mathematically a pulse of strength ϵ, and base b, travelling along the axis of x with a constant velocity v. His formula, true for all values of the distance x, and also for all values of the time t, was

$$\left\{H(x - vt + b) - H(x - vt)\right\}\epsilon$$

It will be seen that this quantity becomes ϵ if the value of $(x - vt)$ lies between $(-b)$ and 0, and that it becomes zero if such value lies outside the range stated.

If the pulse base b is excessively small, and if we use a for vt, this formula becomes

$$b\frac{d}{dx}H(x - vt)\epsilon = bpe^{-ap}H(x)\epsilon$$

where p is used for the differentiator d/dx.

Throughout Heaviside's mathematical work with operators* the factor pe^{-ap} is continually occurring. This shows that his operator method deals with an isolated moving pulse. His complete operator is always expanded into an infinite series of terms involving powers of p. The unit function $H(x)$ is not always mentioned, but is always assumed to be present. The effect is that the terms of the infinite series denote quantities which come into existence at different times. They denote reflections of a single pulse which pass a given point not simultaneously but in succession. The number of reflections undergone by the pulse denoted by a term of the series is different for each term.

Ordinary analysis yields an equivalent series of terms, but these are treated as simultaneous quantities and as mere numbers which can be added together so as to merge into a single one. For most purposes this leads to correct results. The original pulse is only the first of a sequence, the members of which under steady current conditions all have the same strength. The reflected pulses passing a point at a given instant, although not due to the same initial pulse, are exactly the same as if this were the case. Each pulse may affect matter on which it falls, and the joint effect on a portion of matter of a number of such pulses appears to be the same as that of a single pulse equal to the vector sum of all the pulses simultaneously acting on the matter. So far as the effect on matter is concerned the pulses can be added together as vectors, but, if these vectors represent physical entities which are individual and independent, the physical facts are obscured by adding the pulses and merging them into a single one. Moreover, such procedure can readily be shown to be inconsistent with energy considerations if we apply to a small element of volume the energy principles denoted by the words "conservation" and "localization."

The conflict between the various views is a little curious. Mathematicians, with few exceptions, treat the waves as waves of number only. Heaviside took for granted the physical existence of waves consisting of sequences of individual electromagnetic sheets, each moving with the velocity of light, and each suffering reflection at every material surface it met. There is no reason to suppose that such reflections can cease if the source of the fluxes becomes fixed at some steady value, even if this value is zero. On this point, however, Heaviside's writings are not always clear and consistent.† He seems inclined to take the view which even now seems to be the one most commonly held, that when the current becomes steady the fluxes become static. One must bear in mind that Heaviside's work was not concerned with steady current conditions. He was interested only in the initial state of the current and paid next to no attention to steady states.

* The precise definition, and the mathematical properties, of $H(x)$, and also of its derivative $pH(x)$, will be found discussed in "Impulse Functions," and in "Index Operators." (See *Philosophical Magazine*, 1931, vol. 11, p. 345; and vol. 12, p. 201.)

† See "Electromagnetic Theory," vol. 1, p. 310.

An electric current has always been looked upon as a kinetic phenomenon, yet the first suggestion that under steady current conditions the fluxes are moving in space and carrying their energy with them, is to be found in the writings of Poynting. The velocity of movement of the Faraday tubes was in certain cases actually calculated by Poynting, but he could not credit, or explain, the result arrived at. His physical conceptions are suggestive of optical processes, yet he makes no allusion to light, or to the possibility of reflections involving superposed fluxes. When Poynting's theorem was published Maxwell's theory was still devoid of direct experimental evidence. At that time the theory was not very widely held, and, indeed, except in this country, was not regarded with favour. Fluxes under steady current conditions were looked upon as static. Poynting was contesting normal views when suggesting the quite new idea of a steady stream of fluxes. It was too soon to think about superposed streams.

Heaviside, on the other hand, pointed out that the echo-action of reflections was the root cause of the failure of high-speed electrical signalling. He spent much of his life in studying the conditions under which the coefficient of reflection could be made negligible. In general his views suggest the idea that the electrical engineer has been a radio engineer all his life without knowing it.

The mathematics of the matter, given below, involves analysis based on assumptions. It will be found that the analysis is a straightforward example of Heaviside's methods, involving no new point except in the sense that it is applied to a new case. The assumptions made appear to involve merely the application, in a new region, of principles whose truth is already recognized. Before dealing with them it is desirable to point out certain results which follow from the assumption, tacitly made in the proof of Poynting's theorem, that the flux system is single. Some of these results are striking, and do not appear to harmonize with Maxwell's theory. A most startling result was set forth by Poynting himself in his 1885 paper. He offered no explanation of it, and no one appears to have paid any attention to it.

(3) Single Flux Transmission.

In a wave of light $\mathbf{H}_e$ and $\mathbf{H}_m$ are always perpendicular to each other, and the ratio of their magnitudes is constant and corresponds with equal electric and magnetic energy-densities T_e and T_m. Poynting did not assume that either of these relations was necessarily true in cases of electrical power transmission. With light the velocity of each flux is the same and is fixed in amount. In Poynting's general case the velocities V_e and V_m of the electric and magnetic fluxes are normally different from one another and vary from place to place.

Poynting showed how to calculate the values of V_e and V_m at the surface of a long straight conductor traversed by a constant current. He showed that their values depend only on the radius r of the wire, and on its specific resistance, taken as 1 642 electromagnetic units. His formulæ, quoted from the 1885 paper,* are

$$V_e = 2 \times 9 \times 10^{20}\pi r/1\,642 \quad \text{and} \quad V_m = 1\,642/(2\pi\mu r)$$

* J. H. Poynting, "Collected Scientific Papers," p. 202.

The permeability μ must be taken as unity. Poynting does not appear to notice that $V_eV_m = v^2$, where $v = 3 \times 10^{10}$, and he says nothing about numerical values. For a wire of 1 cm diameter $r = 0{\cdot}5$ and $V_e = 57{\cdot}3 \times 10^6 v$, or nearly 60 million times the velocity of light. Poynting did not credit the result and said that "of course these velocities are purely hypothetical." He offered no explanation of them, and passed on to illustrate the flow of electric tubes in certain simple circuits.

Now such a result must have some meaning, and merits further attention. The electromagnetic laws* as arranged symmetrically by Heaviside† can be stated as follows:—

$$\mathbf{B}_e = e\mathbf{H}_e \qquad \mathbf{B}_m = m\mathbf{H}_m \quad . \quad . \quad . \quad (1)$$

$$\overline{\nabla\mathbf{B}_e} = 0 \qquad \overline{\nabla\mathbf{B}_m} = 0 \quad . \quad . \quad (2)$$

$$T_e = \tfrac{1}{2}\mathbf{H}_e\mathbf{B}_e \qquad T_m = \tfrac{1}{2}\mathbf{H}_m\mathbf{B}_m \quad . \quad . \quad . \quad (3)$$

The fourth law can be put in two forms:—

$$[\nabla\mathbf{H}_e] = -\,\dot{\mathbf{B}}_m \qquad [\nabla\mathbf{H}_m] = +\,\dot{\mathbf{B}}_e \quad . \quad . \quad (4c)$$

$$\mathbf{H}_e = [\mathbf{B}_m\mathbf{V}_m] \qquad \mathbf{H}_m = [\mathbf{V}_e\mathbf{B}_e] \quad . \quad . \quad (4v)$$

The Poynting flux is

$$\mathbf{P}_{em} = [\mathbf{H}_e\mathbf{H}_m] \quad . \quad . \quad . \quad . \quad . \quad (5)$$

It readily follows from (1) and (4v) that $\mathbf{H}_e$ is perpendicular to $\mathbf{H}_m$, and that $\mathbf{V}_e$ is parallel to $\mathbf{V}_m$, each being perpendicular to both of the vectors $\mathbf{H}$. Poynting did not acknowledge that the two $\mathbf{H}$ vectors were always at right angles. He acknowledged that the laws (4c) and (4v) were each true and necessary, but he did not regard them as complete or sufficient.

If we take a case which is symmetrical about an axis, it is always true that the H vectors are perpendicular. In any such case it can readily be shown from the above equations that

$$\mathbf{V}_e\mathbf{V}_m = v^2 = 1/(em) \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$\left.\begin{aligned} \mathbf{V}_eT_e &= \mathbf{V}_mT_m \\ \mathbf{P}_{em} &= \mathbf{V}_eT_e + \mathbf{V}_mT_m \end{aligned}\right\} \quad . \quad . \quad . \quad (7)$$

Thus the Poynting flux consists of two equal parts, one of electric, the other of magnetic, energy. Each flux moves on carrying all its energy with it; but the velocity of each flux is inversely proportional to the corresponding energy density. If the velocities are equal to each other, each is equal to v. In all cases we have

$$\left.\begin{aligned} V_e = v\sqrt{(T_m/T_e)} \qquad V_m = v\sqrt{(T_e/T_m)} \\ \frac{V_e}{V_m} = \frac{mH_m^2}{eH_e^2} = \left(vm\frac{H_m}{H_e}\right)^2 = \frac{T_m}{T_e} \end{aligned}\right\} . \quad (8)$$

With a steady current system, assuming that the fluxes are single, it is easy to calculate at certain points the ratios $H_e : H_m$ and $T_e : T_m$. The velocities follow from equations (8).

* The notation, the formulæ, and the vector methods, used in this paper are the same as those used in the Kelvin lecture on the work of Oliver Heaviside (*Journal I.E.E.*, vol. 71, p. 837) and also in "Electromagnetic Waves and Pulses" (*Philosophical Magazine*, 1932, vol. 13, p. 1049).

† *Journal I.E.E.*, 1932, vol. 71, p. 839.

Consider the case of a concentric cable conveying a current of 100 amperes from a generator G at 1 000 volts to a load L taking this current at 900 volts, the voltage drop in each lead being 50 volts. In Fig. 3 let AA′ be the axis, the values of the radius being 0·5 cm for the inner lead, and 2·0 cm and 2·236 cm respectively for the outer one. The two leads, if of the same specific resistance (taken to be 1 642 e.m.u., as in Poynting's case) will then have the same resistance. There will be a fall of 1 volt per 4 760 cm of cable, and the length gl (see Fig. 3) for a total drop of 50 volts will be 2·38 km. The ratio $H_m : H_e$ can easily be calculated at any point on the surface of either lead and also at the generator, assuming the ordinary static distribution of potential between the two leads at the generator end of the cable. The foregoing relations and data yield results as follows:—

At the inner conductor $V_e = 57 \cdot 3 \times 10^6 v$

At the outer conductor $V_e = 14 \cdot 3 \times 10^6 v$

At the generator $V_e = 8 \cdot 34 v$

The corresponding values of V_m follow from equation (6).

By equations (8) the ratio $T_m : T_e$ is the same as the

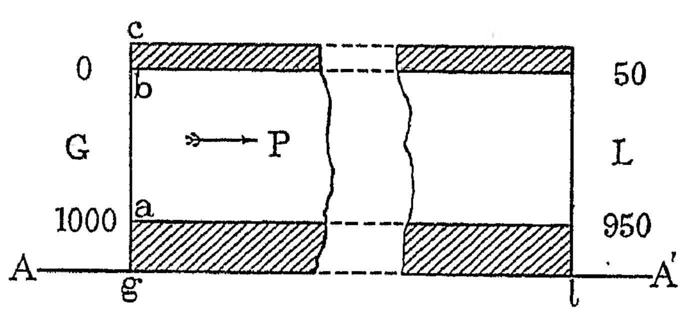

FIG. 3.

square of $V_e : v$, so that the energy densities* in the above three cases are respectively in the ratios

$$3 \cdot 3 \times 10^{15} \; ; \; 2 \times 10^{14} \; ; \; 69 \cdot 5$$

These results follow from the assumption that the fluxes are single. If the facts correspond with superposed light waves we have $T_m = T_e$ for each wave, and thus also for the superposed system.

The above velocities will not be credited as real, yet they seem to be quite consistent with the movement of the fluxes.

Suppose in Fig. 3 that 1 000 electric level surfaces are constructed, one for each volt from 0 to 1 000. These will all be surfaces of revolution about AA′, and, in the plane of the paper, can be represented by lines leaving the radius at the generator at points having the corresponding potentials. The inner 50 will drop into the central lead. The outer 50 will fall into the outer lead. The middle 900 will pass along the 2·38 km of the cable from generator to load without touching either conductor, so that the lines representing them, and also the Poynting flux, will be all essentially parallel to the axis. Each line of H_e must be perpendicular to every surface, so that it will be radial at the 900 inner surfaces, but each end of it must bend round towards the generator asymptotically to the conductor so as to reach the generator either at the 1 000-volt point or at the zero-volt point without touching the conductor anywhere (see also Fig. 5).

* Only the moving fluxes are concerned. With constant current all voltages are fixed, and there must be static electric fluxes which are not concerned with the moving ones, and are outside the present problem, like the magnetic fluxes due to a fixed permanent magnet.

Let ϵ_1 and ϵ_2 in Fig. 4 denote any two electric level surfaces, and let H, H', be two lines of $\mathbf{H}_e$ which are very close together, their distances apart being d_1 along ϵ_1 and d_2 along ϵ_2. Let the distance from the axis AA′ be y_1 for d_1 and y_2 for d_2. Distinguish by an extra

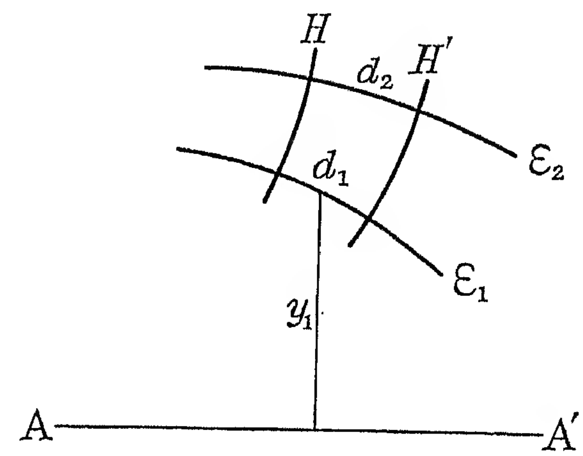

FIG. 4.

suffix, $s = 1$, or $s = 2$, corresponding quantities for the surfaces ϵ_1 and ϵ_2.

Each of the expressions $2\pi y_s d_s e H_{es}$ and $2\pi y_s H_{ms}$ must have the same value for $s = 1$ or 2. This follows in the first case from continuity of the flux, and in the second from the law of the magnetic circuit. Moreover, the equation $H_{ms} = V_{es} e H_{es}$ given by the vector law (4v), must be true whether $s = 1$ or $s = 2$. From these equations it readily follows that

$$d_1/V_{e1} = d_2/V_{e2} \quad . \quad . \quad . \quad . \quad (9)$$

This means that the time taken for a point on the line H_e to reach the corresponding point on the line next

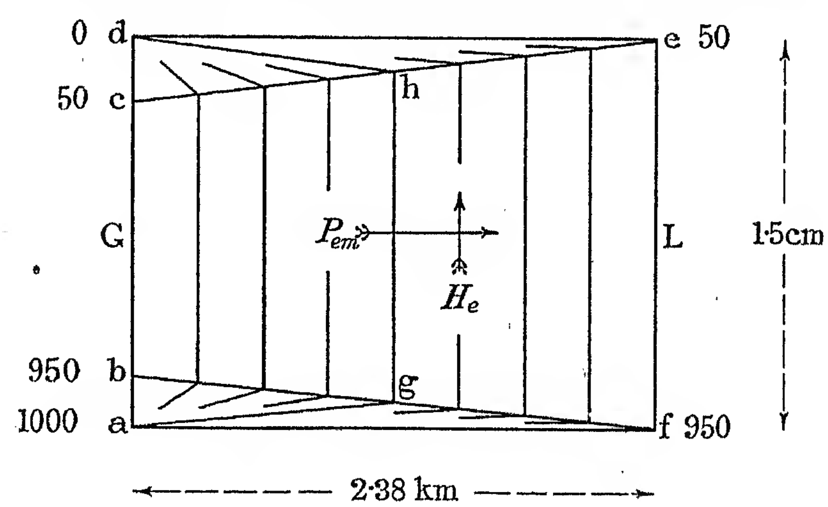

FIG. 5.

ahead of it is the same for each point on the line. It also means that each point on the line H_e reaches the conductor or load at the same instant, since a line drawn from the generator along the bounding surfaces of the outgoing lead, the load, and the return lead, back to the generator, is one of the H_e lines. The first result is what one expects if the fluxes are moving and yet represent what is called a steady state. The second harmonizes perfectly with Poynting's measure of current, as the

rate at which the electric lines enter the conductor. In a steady state this rate must be the same all round the circuit. Such a result does not hold if the current is changing, so that its strength varies with the part of the conductor considered. The H_e line will then lie partly in the conductor and partly in the dielectric, and moreover there may be energy losses in conducting matter isolated in the dielectric. If the currents are steady the H_e line is wholly within the conductor or wholly within the dielectric. Under such conditions there are no dielectric currents.

The H_e lines of Fig. 3 cannot be made clear on a drawing made to scale; but are indicated in Fig. 5. The boundary of the dielectric is *afed*, and a typical H_e line *aghd*, while *ce* and *bf* denote electric level surfaces for 50 and 950 volts. The line integral of H_e is 1 000 for every complete line joining the poles *ad*, and is 900 for the portion *gh*. The length *af* is $1 \cdot 6 \times 10^5$ in terms of *ad*, while *ab* and *cd* are respectively 2 per cent and 10 per cent of *ad*.

Each H_e line emitted from G takes a form which becomes more and more like that of the bounding matter which it is approaching, and reaches this boundary everywhere at the same instant. Now though the closed circuit regulates the output of the tubes it does not affect the e.m.f. Each of these tubes represents a sheet the H_e lines of which are tied to the poles *ad*. The shape of the moving sheet, though it may depend on matter which has been left behind, cannot depend on matter ahead and not yet reached. Everything seems to suggest that the P_{em} solution, though it may be mathematically true as regards the energy distribution to matter, gives a misleading picture of what is actually happening in the dielectric.

The values found for V_e and V_m are incredible as physical entities, but they seem to yield results in accordance with the facts. The question arises whether we are dealing directly with physical entities, or with some mathematical and composite picture of them. If the flux system is not single, the quantities $\mathbf{H}_e$, $\mathbf{H}_m$, and $\mathbf{P}_{em}$, although derived in a definite way from similar quantities denoting real entities, do not themselves represent real quantities. Their existence is only mathematical, and a mathematical abstraction can move at any pace.

(4) The Radiation Assumption.

The physical assumption which we make, and which, in conjunction with Maxwell's theory, appears to define the mathematical problem to be solved, is as follows. Under steady current conditions, with all matter relatively fixed, each travelling electromagnetic disturbance moves and is reflected just like a ray of light, and possesses the same characteristics, including those of individuality and independence. Each such disturbance behaves as if others were non-existent. It can be looked upon as a sequence of fluxes such that as each member passes a given point it is succeeded by another exactly like itself. An endless number of such sequences may be superposed, yet the electrical conditions at any point remain constant in time.

It would be a mistake to suggest that there is anything new about an assumption when this appears to be covered by other assumptions the truth of which is invariably taken for granted, and which in age are more venerable than any theory of electricity. The independence of light waves is a principle which dates back to Huygens, and has always been regarded as obviously true. Broadcast telephony has added most convincing, though possibly needless, confirmation of it. Yet the individuality of light waves does not appear so striking when the waves cross each other's paths, as when they travel along one ray in the same, or in the opposite, sense. Two aerials however far apart can be used with success to transmit simultaneously in each direction a large number of independent messages, some by telegraphic signals, others by telephonic ones. All these signals traverse the same path, and this may involve many reflections from the Heaviside layer. The complicated arrangements of the transmitter and receiver have nothing to do with the mode of propagation of the waves through the ether. The waves must superpose, yet must still retain their separate individuality. If it is thought imaginative to suppose that two simple streams of equal fluxes can cross each other without disturbance, how much more imaginative it must be to suppose that the highly complex waves from each and every point on a star can travel along the same ray to the earth without merging into a single wave and without being disturbed by waves from other stars.

If we consider the case of two electrical circuits, though telephone engineers know to their cost that such circuits can influence one another if the currents are changing, yet, if the currents are constant, the engineer of either circuit can calculate how its energy is distributed without reference to the flow of energy in the other, and can rely on the fact that it is the same as if the current in the other were zero. If the fluxes of each circuit are moving, these fluxes must cross each other in the dielectric. The principle of superposition must hold, yet individuality must be retained. It is the habit of combining the fluxes in a special way which appears to be at fault. Two vector quantities of the same kind may each represent something real, but it does not follow that the sum of the vectors represents anything real, however useful the new vector may possibly be for mathematical purposes. At any point in the joint system there is a Poynting flux due to each circuit, and each represents a real flux of energy in the appropriate direction. The vector sum of these fluxes represents nothing real, and the corresponding direction of energy flow is incorrect. If the fluxes really combine one circuit must influence the other however steady the two currents happen to be; but it is taken for granted, and is known to be true, that no such effect takes place, whatever the relative value of the two currents, and hence whatever the relative value of the two corresponding fluxes at a particular point.

One point about the assumption mentioned above concerns the reflection of disturbances, and may seem to involve the properties of wave motion, and of matter. Actually all that is assumed is that like causes under like conditions produce like effects. If a sequence of incident disturbances D_1 gives rise to a sequence of reflected disturbances D_2, although the strength and polarization of

D_2 may differ from those of D_1, yet, if each member of the sequence D_1 is the same, so will be each member of the sequence D_2.

Heaviside has stated his views of radio-waves* clearly and briefly. He says, "Electric telegraphy is done by means of electromagnetic waves through the ether differing in no essential respect from radiation from the Sun." A wave is looked upon as a sequence of individual electromagnetic sheets or "slabs." "There may be any number of slabs of the above kind, separated, or in contact, of any depths and any strengths. . . . If E and H do not change in direction from one slab to another, the radiation is plane polarized. . . . The direction of E (and with it that of H) may vary from slab to slab in any way we like. . . . The disturbance is vibratory in solar radiation, but in telegraphy this is not necessary. . . . We may have waves in which E and H do not change at all. It is the progression through the ether that is the wave, not accidental vibration."

There is nothing in Maxwell's theory to suggest a difference in the mode of propagation with change of frequency as we proceed from waves of ordinary light to those used in radio-telephony, to those involved with alternating currents of commercial frequencies, and to others in which the frequency is so low that the corresponding current is essentially steady. In all cases there must be reflections. The sequence of equal pulses associated with steady current conditions is not in conflict with wave motion, but rather illustrates its nature in the case of light. Suppose the e.m.f. of the generator switched on to the cable shown in Fig. 3 is alternating at a frequency of 1 cycle per century, and that after 1 minute it is switched off. The time taken to establish the steady state has nothing to do with the frequency or wave-form of the generator. It is determined by the free oscillations of the system, not by the forced ones. For the cable in Fig. 3 this time is certainly less than 1 second. Thus for 59 seconds the steady state holds. The portion of the wave concerned is the ratio of this time to 100 years, or 1 to 5×10^7. The wavelength, of 100 light-years, is greater than the distance of the fainter stars, and can have little to do with a problem confined to a cable less than $2\frac{1}{2}$ km long and less than 5 cm in diameter. Any wave motion that occurs is devoid of a single wave and is best described as a sequence of Heaviside pulses. If there is any minimum depth natural to these pulses it is probably excessively small,† but, even if it is as much as a millimetre, the generator emits 3×10^{11} of them per second, and all of these are essentially of the same strength. Each pulse traverses the length of the cable about 10^5 times per second, so that, even if the reflections only occur at the ends of the cable, each point in the dielectric is traversed simultaneously by pulse sequences, the number of which is immense and is continually increasing while the generator is in action. Since reflections occur all along the cable the Poynting fluxes at any point are even more numerous, and involve all directions of flow.

Maxwell's equations can as easily be used to explain the movement of a single pulse as that of a sequence of complete waves. The subject of pulses is, moreover, not entirely devoid of experimental evidence. No one supposes that the velocity of light in the ether depends upon frequency. Experimental proof may not be very complete, but astronomical facts are held to give satisfactory confirmation. If the velocity is independent of frequency it must be independent of wave-form, and this wave-form may correspond with an isolated train of pulses or even with a single pulse. Experiments on short trains of pulses have been made. The earlier forms of radio-telegraphy, using spark discharges, dealt with an isolated train of highly damped vibrations, each train being a few wavelengths long. More recent tests have been made by Appleton and others, on reflections from the Heaviside layer, using the cathode-ray oscillograph, and "pulse generators" emitting short pulses lasting only for 0·0001 second, though still several wavelengths long. The striking experiments of Quäck on round-the-world echoes show that an isolated train of pulses is propagated just like a continuous sequence of waves of light. In particular the numerous tests on atmospherics conducted under the general supervision of the Radio Research Board* seem to show that many of these impulsive disturbances are, in the main, undirectional in character. They give experimental evidence in favour of the view that an isolated unidirectional pulse travels, and is reflected, just like a complete wave.

A good illustration of some of the points above referred to is to be found in a striking paper by Zworykin entitled "Television with Cathode-Ray Tubes."† Fig. 21 of that paper is reproduced here as Fig. 6. The three independent sets of radio signals transmitted from aerial to aerial consist of the highly complex wave (B) of the picture signal, and two sets of impulses C and D, needed for synchronizing the horizontal and vertical deflecting apparatus used for scanning the picture. Each set of synchronizing signals is due to a "saw-tooth generator" involving a special arrangement of condenser and discharge tube, and producing periodically what are essentially miniature lightning flashes or atmospherics. During most of the cycle the generator voltage is minute and positive, but for a small fraction of the period it is large and negative. Each signal is transmitted like an atmospheric, affecting the receiver in spite of any tuning or detuning of the circuits. It travels with the carrier wave but maintains its own individuality. It neither influences, nor is influenced by, the modulation of the carrier wave due to the picture signals. It is produced by the apparatus accessory to the cathode ray used for scanning the picture at the emitter, and controls the scanning of the cathode ray producing the picture at the receiver, but it does not affect the lighting of the parts of this picture. It is one of a set of pulses which are essentially unidirectional.

It is pointed out that even "the irregular-shaped picture signals . . . are often unsymmetrical about the

* See "Electromagnetic Theory," vol. 3, p. 333 (an article on telegraphy reprinted from the "Encyclopædia Britannica," 10th edition).

† *Philosophical Magazine*, 1932, vol. 13, p. 1073.

* Reference should be made to the Reports of the Board for 1929 (pp. 90–91), and for 1930 (pp. 26, 31, and 58–63). The extraordinary wave-forms given on page 91 of the first Report imply that the disturbances are not only of very short duration, but also that many of them are in the main unidirectional in character. On page 31 of the 1930 Report tests are given of the "equivalent path" of the radio-waves directed upwards and reflected back to earth by the Heaviside layer. This path is found to be $(\alpha + \beta f)$, where α and β are constants and f is the frequency of the waves used. Tests were made with values of f varying from 800 to 3 000 kilocycles per second. The linear relation was verified, the value of βf being in all cases much less than α. Extrapolation is always doubtful, but the simplest inference from this result is that if tests could be made with $f = 0$, the equivalent path found would be α.

† *Journal I.E.E.*, 1933, vol. 73, p. 437.

axis, usually being more positive than negative. Both synchronizing signals are arranged to have their peaks on the negative side of the axis." The "difference in wave-shape is utilized at the receiver for the purpose of separating these two synchronizing signals." "A simple filter in each of the input circuits of the two deflecting units gives satisfactory discrimination against undesired synchronizing impulses. The plate circuits of both dynatron input tubes contain circuits approximately resonant to the operating periods of their respective deflecting circuits, thus aiding in the matter of selectivity."

The modulator is not described. Apparently each set of signals passes through it. Now it is only too well known in broadcast telephony that impulsive disturbances, like atmospherics, can get through any combination of coils, condensers, and electron tubes. Resonant conditions, and impedance formulæ, only apply to the cyclic state, or to such a state when successive amplitudes are slowly changed by modulation like that due to the picture signals. The modulation is not affected by the impulsive signals, each of which must be radiated from the aerial independently of the others. Such signals consist of isolated pulses, and there seems no reason to suppose that they differ from light either in physical nature or in mode of propagation.

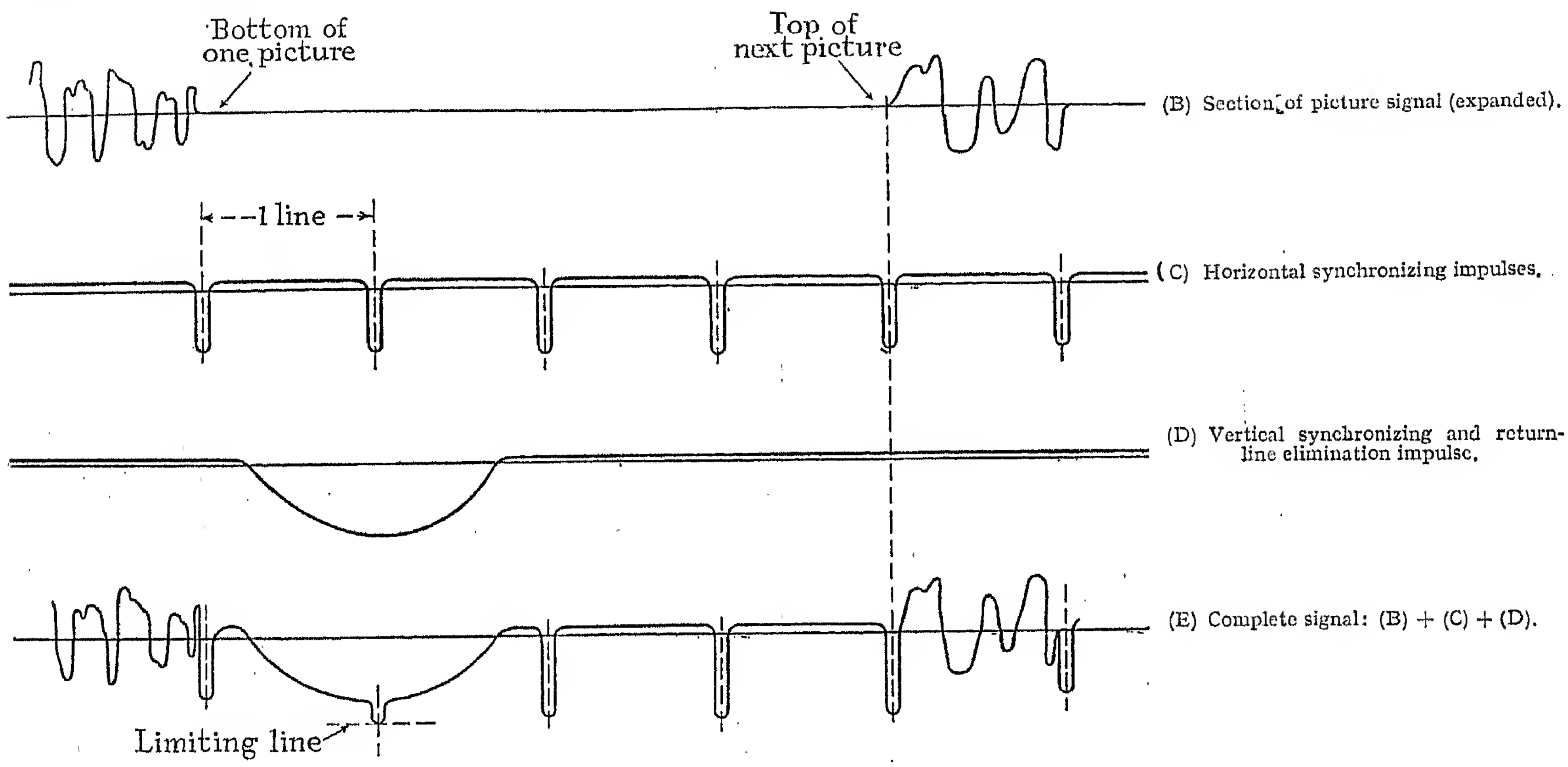

FIG. 6.

(5) THE LOCAL-IMPULSE ASSUMPTION.

The mathematical assumptions we shall make are embodied in the following statement. Under steady current conditions, with all matter relatively fixed, $\dot{\mathbf{B}}_e$ and $\dot{\mathbf{B}}_m$ are zero except in the very close neighbourhood of the mass-points giving rise to them. In all cases $\dot{\mathbf{B}}_e$ and $\dot{\mathbf{B}}_m$ are circuital vectors.

The ordinary solution of a steady current circuit specifies the way in which the energy is distributed to the circuit, but is not concerned with this energy after it has been given up to the matter. This also holds for the Poynting vector solution. An equivalent solution in terms of superposed Poynting vectors must similarly be independent of any after-history of the energy delivered to the matter. The problem is thus a dielectric one, independent of the behaviour of matter, provided that the reflected waves are taken into account. The analysis must, however, involve volume integrals through spaces containing matter, and it becomes necessary to formulate mathematical assumptions to cover the interchange of energy between the fluxes and the matter.

We adopt the views* of Poynting in reference to (i) the electric current and (ii) the energy and mode of propagation of induction tubes. We also adopt the development of these ideas due to Sir J. J. Thomson.

Poynting supposed, as illustrated in Figs. 3 and 5, that the generator emits induction tubes into the dielectric, through which they pass until they enter the conductor. The number of such tubes which enter the conductor per second is the current I. If the e.m.f. of the generator is E, its power output is EI, so that E represents the energy of each induction tube. Sir J. J. Thomson suggested the idea of "unit tubes," which we shall call "elements." The number of elements in each induction tube is measured by E. If the unit tube, or element, is deprived of its energy it closes up to a ring of infinitesimal size. If the closed ring is energized it is opened out, the associated quantity of energy being a fixed natural unit the same for each element.

This definition of current is only a mathematical measure of it in terms of induction tubes. It suggests

* See the 1884 and 1885 papers of Poynting (*Philosophical Transactions of the Royal Society*, 1884, p. 343, and 1885, p. 277); and also Poynting's "Collected Scientific Papers," pp. 172, 224 ("The Discharge of Electricity through an Imperfect Insulator"), and 271 ("Molecular Electricity"). The last paper "may be regarded as a theory of the conservation of induction tubes, and of their beginning and ending on atoms." The developments due to Sir J. J. Thomson are given in the early pages of his "Recent Researches in Electricity and Magnetism," 1893.

nothing as to the physical nature of current. Poynting's view was that the induction tube, when passing through matter, could give up its energy to the matter, and vanish in the act. In terms of Thomson unit tubes the induction tube could split up into its elements, each of which could deliver up its energy to the matter, and by so doing could close up to infinitesimal size. If an element is energized its energy is fixed, but not its length. What Poynting called the "energy length" of a tube was not its length, but the energy of its length, and was a measure of the number of elements within it, such elements giving the tube a cellular structure.

Poynting's words were "the wire is not capable of bearing a continually increasing induction, and breaks the tubes up, as it were, their energy appearing finally as heat." "May we not say that the tubes are dissolved?

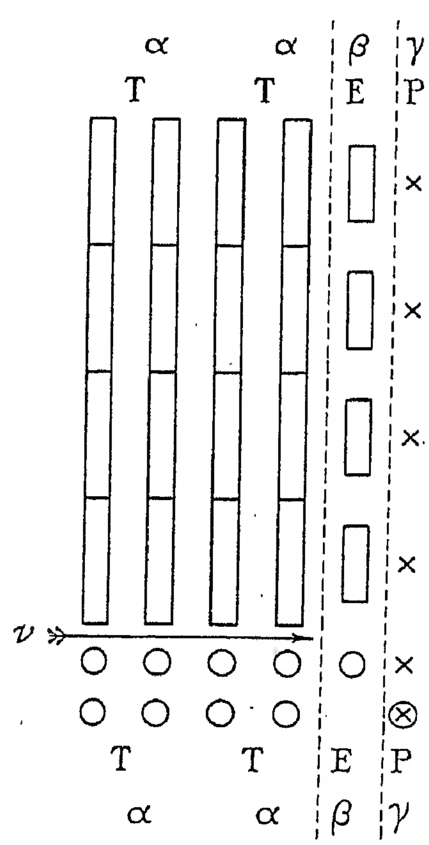

FIG. 7.

The term seems to suggest that the induction is not destroyed, but only loses its continuity."

Under steady current conditions all quantities must be constant. The rate at which lines enter the conductor must be equal to the rate at which they vanish within the conductor. The latter process represents the physical nature of the current, yet the former rate may be used as the numerical measure of the current, since the two rates are equal. The density of the lines remains constant within the conductor, and it is to be noted that a particular line does not necessarily disappear the instant it enters the conductor.

If by $\dot{\mathbf{B}}_e$ we always mean the net rate at which lines enter the unit area from outside, $\dot{\mathbf{B}}_e$ can always be considered to be the current density, whether in the dielectric or in the conductor. In the dielectric it is zero under steady current conditions, but not if the currents are changing. Under the latter conditions lines may enter the conductor at a different rate from that at which they vanish within it. These ideas work out consistently, but there seems no need here to go into details. In the main such consistency was explained in Poynting's original paper.

For present purposes it seems possible, without undue straining, to develop the foregoing ideas a stage further. Suppose, as depicted in Fig. 7, that the electric tubes T are moving at right angles to their lengths, with a velocity v, through a region containing matter. Distinguish three parts, α, β, γ, of this region as shown separated by the dotted lines. We assume: (i) that in the region α, although there may be matter, this matter at the moment is in an inactive state and does not affect the tubes or lines T; (ii) that the region β is very close to the mass-points P, in the region γ, denoting matter which is in some way polarized and in a condition to absorb energy; and that the tube E in β separates into its elements; and (iii) that each element of E in β closes up impulsively round the corresponding mass-point in γ. The lower part of the figure is intended to picture this process, looking at the lines end-on.

Modern theories of matter suggest that its ultimate particles are excessively small compared with the distances which separate them. Mathematically these particles can be regarded as points, which we may call mass-points. They are supposed to exist in different energy states. The change from one state to another is regarded as impulsive, so that it lasts only an infinitely short time, during which the mass-point must act either as a source or as a sink of energy. These views do not appear to conflict in any way with those due to Poynting and Thomson, in accordance with which mass-points can absorb, or emit, Thomson elements. The process indicated in Fig. 7 is easily imagined reversible. A battery or generator may act by polarizing its mass-points, so that they are able to energize closed tubes into elements, and to line them up and emit them as tubes.

Now consider the induction density $\mathbf{B}$ (whether $\mathbf{B}_e$ or $\mathbf{B}_m$) in the regions α, β, γ, of Fig. 7. In region α the value of $\mathbf{B}$ is unaffected and under steady current conditions is constant, so that $\dot{\mathbf{B}}$ is zero; but in regions β and γ it seems natural to expect that the impulsive change of the element in β to the closed tube in γ must disturb locally the flux density $\mathbf{B}$, and that in these regions $\dot{\mathbf{B}}$ cannot be assumed zero. It is, however, a natural inference that $\dot{\mathbf{B}}$ differs from zero only in the very close neighbourhood of the mass-point giving rise to its existence. For present purposes this is the important part of the assumption we make. That $\dot{\mathbf{B}}$ is always a circuital vector follows from assuming (i) Maxwell's principle, that currents always flow in closed circuits, and (ii) that the principle holds however small the dimensions of these circuits.

(6) Summary of Results.

With the aid of the assumptions that the current density $\dot{\mathbf{B}}$ is always circuital, and that under steady current conditions it is zero except in the very close neighbourhood of the sources or sinks of energy; it is possible, as shown in Part 2, to justify the following statement.

Under steady current conditions, with all matter

relatively fixed, the transmission of energy in an electrical circuit takes place by a superposed system of radiation fluxes, each flux having all the properties of a flux of light. The distribution of energy to the circuit by these fluxes is the same as that given by the Poynting flux.

This may be expressed mathematically thus. If the radio-waves associated with a steady current system of distribution are typified by

$$\mathbf{H}_{es},\quad \mathbf{H}_{ms},\quad \mathbf{P}_s = [\mathbf{H}_{es}\mathbf{H}_{ms}],$$

and if

$$\mathbf{H}_e \equiv \sum_s \mathbf{H}_{es},\quad \mathbf{H}_m \equiv \sum_s \mathbf{H}_{ms},\quad \mathbf{P}_{em} \equiv [\mathbf{H}_e\mathbf{H}_m],$$

then, for any completely closed surface of which $d\sigma$ is a vector element,

$$\left.\begin{aligned} &\iint\overline{\mathbf{P}_{em}d\sigma} = \sum_s\iint\overline{\mathbf{P}_s d\sigma} \\ \text{or}\quad &\iint\overline{[\mathbf{H}_e\mathbf{H}_m]d\sigma} = \sum_s\iint\overline{[\mathbf{H}_{es}\mathbf{H}_{ms}]d\sigma} \end{aligned}\right\} \quad . \quad . \quad (10)$$

A similar proposition is true, as a time average, for cyclic current conditions.

It is to be noted that we have definitely excluded from consideration what happens while the conditions of the circuit alter from one steady state to another. In each of these states there are static electric fluxes with energy represented by Maxwell electric cells. How this energy changes between two such states is not here considered.

Static fluxes seem to involve complexities as great as those due to moving ones, and may possibly be connected with the way the energy is guided by the circuit to the load. This guidance is said to be explained by means of the Poynting flux $\mathbf{P}_{em}$. The explanation is mathematical rather than physical. Can a formula, however precise, really explain anything physical? An explanation in terms of superposed fluxes $\mathbf{P}_s$, each having the properties of light, is even more difficult, and is not here attempted. We have assumed the individuality of fluxes, but we have also assumed continuity, so that each flux forms part of an electromagnetic sheet. The pulses in the sheet are controlled in polarization by the lines of $\mathbf{H}_e$ and $\mathbf{H}_m$. The sheets are independent of one another, but the pulses in the sheet must conform to the properties of the sheet. Each sheet is a net the meshes of which are formed by the $\mathbf{H}_e$ and $\mathbf{H}_m$ lines, and the crossing of these lines represents the polarized $\mathbf{B}_e$ and $\mathbf{B}_m$ fluxes of the pulse. The pulses are not affected by matter in a way describable as direct action at a distance, but are yet controlled by the $\mathbf{H}_e$ and $\mathbf{H}_m$ lines, the former of which presumably extend to matter. When a sheet is incident on matter it splits up into a transmitted, and a reflected, sheet, each of the *three* sheets being bounded on the matter by a closed line the area defined by which is continually increasing. However difficult it may be to trace what happens, it seems that the sheets and the corresponding fluxes must be controlled by the matter. The energy of the reflected sheet is less than that of the corresponding part of the incident one, and, after continued reflections, must be reduced to zero. The matter of the closed circuit must ultimately receive all the energy.

A point source of light is supposed to emit radiation symmetrical about any line through the point. An electromagnetic sheet may be symmetrical about a line representing the polarization of the mass-points at the source, the $\mathbf{H}_e$ lines proceeding from pole to pole as in the generator of Fig. 3. Such a sheet cannot be symmetrical about every line through the point. To get such a result we must assume systems superposed. Strange as it may seem, it is conceivable that the further study of the steady current circuit may help to explain what is happening at sources of light, and also what is happening when radio-waves converge on matter.

PART 2.

(7) Assumptions and Theorems Used.

It will be convenient, before dealing with the analysis, to collect the mathematical assumptions made, and the chief theorems required. The physical assumptions about fluxes form no part of the mathematical problem. They simply help to define it.

The mathematical assumption made is that under steady current conditions $\dot{\mathbf{B}}_e$ and $\dot{\mathbf{B}}_m$ are each zero except very close to the mass-points giving rise to them. In all cases they are circuital vectors. . . (11)

We shall use the ordinary properties of vectors, and in particular the theorem—true for any two vectors α and β—

$$\overline{\nabla[\alpha\beta]} = \overline{\beta[\nabla\alpha]} - \overline{\alpha[\nabla\beta]} \quad . \quad . \quad . \quad (12)$$

we shall also want the divergence theorem

$$\iint\overline{\mathbf{F}d\sigma} = \iiint\overline{\nabla\mathbf{F}}dv \quad . \quad . \quad . \quad . \quad (13)$$

where $\mathbf{F}$ is a vector surface density, and $d\sigma$ is a vector element of a surface completely enclosing a volume v of which dv is a scalar element.

If $\mathbf{F}$ is a circuital vector, $\overline{\nabla\mathbf{F}}$ is zero and each integral vanishes. It follows that:—

If $\mathbf{C}$ is a circuital vector, tubes formed with lines always parallel to $\mathbf{C}$ are in all cases re-entrant. The surface integral of $\mathbf{C}$ across any such tube is constant for every section of the tube. (14)

It follows from (14) that if dv is an element of volume of a thin re-entrant tube, whose vector length is $d\lambda$ measured along the tube, the vector $\mathbf{C}dv$ is $kd\lambda$, where k is a constant for the tube. The integral of this vanishes as a vector for the complete tube, and since the whole of space can be portioned out into such tubes, we have, for *any* circuital vector,

$$\iiint\mathbf{C}dv = 0 \quad . \quad . \quad . \quad . \quad . \quad (15)$$

as a vector summation for the whole of space. If $\mathbf{F}$ is a vector which does not vary with position we have a zero value for the scalar integral

$$\iiint\overline{\mathbf{F}\mathbf{C}}dv = \mathbf{F}\iiint\mathbf{C}dv = 0$$

This will not hold in general if $\mathbf{F}$ is a vector function of position; but if $\mathbf{C}$ is either $\dot{\mathbf{B}}_e$ or $\dot{\mathbf{B}}_m$ in the sense of (11), so that it is zero except in the very close neighbourhood of the mass-point to which it is due, and

if $\mathbf{F}$ is a vector function of position which is not dependent on the value of $\mathbf{C}$, the above integral must vanish, because $\mathbf{C}$ is in general zero, and only exists in a very small region throughout which $\mathbf{F}$ can be assumed invariable.

It follows that if $\dot{\mathbf{B}}_{es}$ and $\dot{\mathbf{B}}_{ms}$ are circuital vectors due to the action of a mass-point s, and of zero value except very close to the point s, and if $\mathbf{H}_{er}$ and $\mathbf{H}_{mr}$ are vector functions of position associated with a mass-point r, and independent of what is happening at the point s, we have, as scalar quantities,

$$\left.\begin{aligned} \iiint \overline{\mathbf{H}_{er}\dot{\mathbf{B}}_{ms}}dv &= \mathbf{H}_{er}\iiint \dot{\mathbf{B}}_{ms}dv = 0 \\ \iiint \overline{\mathbf{H}_{mr}\dot{\mathbf{B}}_{es}}dv &= \mathbf{H}_{mr}\iiint \dot{\mathbf{B}}_{es}dv = 0 \end{aligned}\right\} \quad . \quad (16)$$

These results will not follow if the mass-points r and s are the same. In such a case the values of $\mathbf{H}$ and $\mathbf{B}$ are connected. The integrals are still confined to the immediate neighbourhood of the corresponding mass-points, but, as shown later, determine the rates at which energy is being taken in at, or sent out from, the mass-points.

(8) Superposed Steady Streams.

We are now in a position to show that the known distribution of energy in steady current systems of supply is the same as that which results if the generator is assumed to act as a system of mass-points emitting a stream of electromagnetic pulses all of equal strength, of constant polarization, and with all the properties of light. Owing to reflections a superposed system of streams results. Let one of a superposed system of such streams be denoted by the constant vector functions of position: $\mathbf{H}_{es}$, $\mathbf{H}_{ms}$, and $\mathbf{P}_s = [\mathbf{H}_{es}\mathbf{H}_{ms}]$. Since each stream has the properties of light,*

$$\left.\begin{aligned} \overline{\nabla\mathbf{H}_{es}} &= 0, & \overline{\nabla\mathbf{H}_{ms}} &= 0 \\ \overline{\mathbf{H}_{es}\mathbf{H}_{ms}} &= 0, & e\mathbf{H}_{es}^2 &= m\mathbf{H}_{ms}^2 \end{aligned}\right\} \quad . \quad (17)$$

Since away from the mass-point the stream is steady,

$$\dot{\mathbf{B}}_{es} = 0, \quad \dot{\mathbf{B}}_{ms} = 0 \quad . \quad . \quad . \quad . \quad (18)$$

It follows that

$$[\nabla\mathbf{H}_{es}] = 0, \quad [\nabla\mathbf{H}_{ms}] = 0, \quad \overline{\nabla\mathbf{P}_s} = 0 \quad . \quad . \quad (19)$$

since the first two relations follow from the circuital law (4c), and the third from (12) by putting $\alpha \equiv \mathbf{H}_{es}$, $\beta = \mathbf{H}_{ms}$, and using the two relations just found.

We can define a single mathematical system of forces and fluxes by the equations

$$\left.\begin{aligned} \mathbf{H}_e &\equiv \sum_s \mathbf{H}_{es}, & \mathbf{H}_m &\equiv \sum_s \mathbf{H}_{ms} \\ \mathbf{P}_{em} &\equiv [\mathbf{H}_e\mathbf{H}_m], & \mathbf{P}^\sigma &\equiv \sum_s \mathbf{P}_s \end{aligned}\right\} \quad . \quad . \quad . \quad (20)$$

As has been previously explained, what we have to show if

$$\mathbf{T} \equiv \mathbf{P}_{em} - \mathbf{P}_\sigma \quad . \quad . \quad . \quad . \quad . \quad (21)$$

is that $\mathbf{T}$ is a circuital flux. This can be done, in spite of the presence of matter, by assuming (11) in the form stated in equations (16).

From simple superposition with the aid of (17), (19), and (20), we have, for any region free from matter,

$$\left.\begin{aligned} \overline{\nabla\mathbf{H}_e} &= 0, & \overline{\nabla\mathbf{H}_m} &= 0, & \overline{\nabla\mathbf{P}_\sigma} &= 0 \\ [\nabla\mathbf{H}_e] &= 0, & [\nabla\mathbf{H}_m] &= 0, & \overline{\nabla\mathbf{P}_{em}} &= 0 \end{aligned}\right\} \quad . \quad (22)$$

The proof of the last relation involves the use of (12) with $\alpha \equiv \mathbf{H}_e$, $\beta = \mathbf{H}_m$, etc.

It is to be noted that we do not get either $\overline{\mathbf{H}_e\mathbf{H}_m} = 0$, or $e\mathbf{H}_e^2 = m\mathbf{H}_m^2$. Moreover, while for any region free from matter we have a zero value for each of the quantities $\overline{\nabla\mathbf{P}_s}$, $\overline{\nabla\mathbf{P}_\sigma}$, $\overline{\nabla\mathbf{P}_{em}}$, and $\overline{\nabla\mathbf{T}}$ (the last following from the use of equation 21), such results do not hold at any mass-point acting as a source or sink of energy. At any such point s the value of $\overline{\nabla\mathbf{P}_\sigma}$ reduces to that of $\overline{\nabla\mathbf{P}_s}$, and each is the rate of output, or input, of energy at the source, or sink, s.

Now let us consider the surface integrals $\iint \overline{\mathbf{P}_{em}d\boldsymbol{\sigma}}$ and $\iint \overline{\mathbf{P}_\sigma d\boldsymbol{\sigma}}$, taken over any completely closed surface of which $d\boldsymbol{\sigma}$ is a vector element. Let us define a quantity Z by the equation

$$\iint \overline{\mathbf{P}_{em}d\boldsymbol{\sigma}} = Z + \iint \overline{\mathbf{P}_\sigma d\boldsymbol{\sigma}} \quad . \quad . \quad . \quad (23)$$

so that from equation (21) we have $Z = \iint \overline{\mathbf{T}d\boldsymbol{\sigma}}$. We have to show that Z is zero.

From equations (20), using elementary vector properties,

$$\mathbf{P}_{em} \equiv [\mathbf{H}_e\mathbf{H}_m] = \sum_r\sum_s [\mathbf{H}_{er}\mathbf{H}_{ms}]$$

where r and s can each have all values, including cases for which $r = s$.

$\mathbf{P}_\sigma$ represents the same summation provided that we include only the cases for which $r = s$. Hence we have, from equation (23),

$$Z = \iint \overline{\mathbf{T}d\boldsymbol{\sigma}} = \sum_r\sum_s \iint \overline{[\mathbf{H}_{er}\mathbf{H}_{ms}]d\boldsymbol{\sigma}} \quad . \quad . \quad (24)$$

where r and s may each have any values provided that we exclude those cases for which $r = s$. If we use the divergence theorem (13), we get

$$Z = \sum_r\sum_s \iiint \overline{\nabla[\mathbf{H}_{er}\mathbf{H}_{ms}]}dv \quad . \quad . \quad . \quad (25)$$

and on using equation (12) with $\alpha \equiv \mathbf{H}_{er}$, $\beta \equiv \mathbf{H}_{ms}$, we get

$$Z = \sum_r\sum_s \iiint \left\{\overline{\mathbf{H}_{ms}[\nabla\mathbf{H}_{er}]} - \overline{\mathbf{H}_{er}[\nabla\mathbf{H}_{ms}]}\right\}dv \quad . \quad (26)$$

Now, if the volume considered does not include any matter, it follows from equations (19) that Z vanishes.

If the volume includes mass-points acting as sources or sinks of energy we shall still prove that Z is zero if we can show that each integral term is zero on the assumption that r is not the same as s.

A typical pair of integrals taken from equation (26) referring to the space variation of the forces associated

* It may be well to point out that the fourth relation in (17), referring to the equality of the electric and magnetic energy densities, is not actually made use of in the analysis which follows.

with a mass-point s, may be stated, without regard to sign, as

$$\iiint \overline{\mathbf{H}_{mr}[\nabla\mathbf{H}_{es}]}dv \quad \text{and} \quad \iiint \overline{\mathbf{H}_{er}[\nabla\mathbf{H}_{ms}]}dv$$

and these, by the circuital laws (4c), become

$$\iiint \overline{\mathbf{H}_{mr}\dot{\mathbf{B}}_{es}}dv \quad \text{and} \quad \iiint \overline{\mathbf{H}_{er}\dot{\mathbf{B}}_{ms}}dv$$

Now under steady current conditions $\dot{\mathbf{B}}_{es}$ and $\dot{\mathbf{B}}_{ms}$ are zero except in the very close neighbourhood of the mass-point s, so that if this mass-point is not within the volume the corresponding integrals vanish, while if the point is included the integrals are restricted to a region infinitely close to s. If r and s are different the vectors $\mathbf{H}_{er}$ and $\mathbf{H}_{mr}$ are not dependent in any way on what is happening at s, and can be regarded as constant over the small region near s. We can thus apply equations (16) to prove that the integrals vanish. The value of Z as defined in equations (24) and (26) is thus zero, and we establish equations (10) for superposed streams of radiation corresponding with a steady state:—

$$\left.\begin{aligned} \iint \overline{\mathbf{P}_{em}d\sigma} &= \sum_s \iint \overline{\mathbf{P}_s d\sigma} \\ \text{or} \qquad \iint \overline{[\mathbf{H}_e\mathbf{H}_m]d\sigma} &= \sum_s \iint \overline{[\mathbf{H}_{es}\mathbf{H}_{ms}]d\sigma} \end{aligned}\right\} \quad . \quad (10)$$

(9) Superposed Cyclic Streams.

By a cyclic stream is meant a stream of pulses each constant in polarization, and such that at any selected point the strength of the pulse is periodic. The fundamental period T is assumed to be the same for each stream.

Even with ordinary light the polarization represented by the vectors $\mathbf{H}$ can be taken as constant, since experiments show that interference effects can be obtained with light pulses from the same source, although emitted from the source at moments many thousands of periods apart. An alternator supplying current to a material circuit must emit pulses that are rigidly constant in polarization. In a pulse of light $\mathbf{H}_{es}$ and $\mathbf{H}_{ms}$ are at right angles and are fixed in ratio at every point of the path. They must therefore have the same waveform when regarded as cyclic functions of the time. Thus we can always put

$$\mathbf{H}_{es} = u_s\mathbf{F}_{es}, \quad \mathbf{H}_{ms} = u_s\mathbf{F}_{ms} \quad . \quad . \quad (27)$$

where $\mathbf{F}_{es}$ and $\mathbf{F}_{ms}$ are vector functions of position independent of time, and perpendicular to each other; and where u_s is a cyclic function of time only, and such that, for every value of s,

$$\frac{1}{T}\int_0^T u_s^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad (28)$$

The forces $\mathbf{F}$ are as continuous as the forces $\mathbf{H}$, and, so far as time variation is concerned, each $\mathbf{F}$ is the r.m.s. value of the corresponding $\mathbf{H}$, so that under cyclic current conditions it is constant at each point. Thus equations (17) must be true of the quantities $\mathbf{F}$, and we have

$$\left.\begin{aligned} \overline{\nabla\mathbf{F}_{es}} &= 0, & \overline{\nabla\mathbf{F}_{ms}} &= 0 \\ \overline{\mathbf{F}_{es}\mathbf{F}_{ms}} &= 0, & e\mathbf{F}_{es}^2 &= m\mathbf{F}_{ms}^2 \end{aligned}\right\} \quad . \quad . \quad (29)$$

If the e.m.f. of the alternator is a pure sine wave, u must be the same for each sequence of pulses however many reflections occur; but if this is not the case we cannot assume that u is the same for different sequences, although we can always use a suitable numerical factor with u to make equation (28) true. The ordinary theory of reflection asserts that the reflecting coefficient may vary with frequency, so that if u_r corresponds with an incident and u_s with a reflected wave we cannot assume that u_r and u_s denote the same wave-form. This will not affect the constancy of the polarization of each member of any sequence $\mathbf{H}$, since, whatever the reduction of strength or the change of polarization due to reflection may be, they will be the same for each member of the sequence. Fluxes defined by the forces $\mathbf{F}$ represent a superposed system of steady streams such as that previously considered. If we use letters $\mathbf{Q}$ to represent the Poynting fluxes of the $\mathbf{F}$ system in the same way as the letters $\mathbf{P}$ were previously used for the $\mathbf{H}$ system, we find that, for any single stream, $\mathbf{Q}$ is the mean value of the corresponding $\mathbf{P}$, but that this does not hold for the compound $\mathbf{F}$ system unless u is the same for every stream.

It follows from equations (27) that

$$\mathbf{Q}_s \equiv [\mathbf{F}_{es}\mathbf{F}_{ms}], \quad \text{where } \mathbf{P}_s = [\mathbf{H}_{es}\mathbf{H}_{ms}] \equiv \mathbf{Q}_s u_s^2$$

Moreover, if

$$\mathbf{Q}_\sigma \equiv \sum_s \mathbf{Q}_s \quad \text{and} \quad \mathbf{P}_\sigma \equiv \sum_s \mathbf{P}_s$$

we have similarly that $\mathbf{Q}_\sigma$ is the mean value of $\mathbf{P}_\sigma$, so that $\mathbf{Q}_s$ is the average value of $\mathbf{P}_s$, by equation (28). Now the $\mathbf{F}$, $\mathbf{Q}$ system is a superposed system of steady streams, so that from equations (19) we have

$$[\nabla\mathbf{F}_{es}] = 0, \quad [\nabla\mathbf{F}_{ms}] = 0, \quad \text{and} \quad \overline{\nabla\mathbf{Q}_s} = 0 \quad . \quad (30)$$

A compound system corresponding with the fluxes $\mathbf{F}$, $\mathbf{Q}$, can be defined as

$$\mathbf{F}_e \equiv \sum_s \mathbf{F}_{es}, \quad \mathbf{F}_m \equiv \sum_s \mathbf{F}_{ms}, \quad \mathbf{Q}_{em} \equiv [\mathbf{F}_e\mathbf{F}_m] \quad . \quad (31)$$

It will be seen that $\mathbf{Q}_{em}$ is not the same as the mean value of $\mathbf{P}_{em}$ unless all the quantities u are the same.

For the steady system $\mathbf{F}$, $\mathbf{Q}$, we must have as before

$$\iint \overline{\mathbf{Q}_{em}d\sigma} = \iint \overline{\mathbf{Q}_\sigma d\sigma}$$

Now since $\mathbf{Q}_\sigma$ is the mean value of $\mathbf{P}_\sigma$, and $\mathbf{P}_\sigma$ is the sum of the vectors $\mathbf{P}_s$, we have

$$\iint \overline{[\mathbf{F}_e\mathbf{F}_m]d\sigma} = \frac{1}{T}\int_0^T dt \sum_s \iint \overline{[\mathbf{H}_{es}\mathbf{H}_{ms}]d\sigma} \quad . \quad . \quad (32)$$

If all the quantities u are the same,

$$\frac{1}{T}\int_0^T dt \iint \overline{[\mathbf{H}_e\mathbf{H}_m]d\sigma}$$

is equal to each element of the equation (32). If all the fluxes are steady, so that each u is unity, equation (32) reduces to equations (10).

The second element of equation (32) represents the time average of the vector sum of the actual Poynting

energy fluxes; while the first element refers to a single mathematical Poynting energy flux determined by $\mathbf{F}_e$ and $\mathbf{F}_m$, two vector functions of position calculated from the individual forces representing the actual streams forming the compound system. The ordinary solutions of alternating-current problems are given in terms of quantities deducible from $\mathbf{F}_e$ and $\mathbf{F}_m$. The vector $\mathbf{Q}_{em}$ represents, as a time mean, the effective flow of energy from matter to matter, although each individual unit of energy travels through the dielectric as a pulse in an electromagnetic sheet represented by $\mathbf{H}_{es}$ and $\mathbf{H}_{ms}$.

APPENDIX.

Currents and Closed Circuits.

The working conditions of steady current circuits were settled, in the main, before any clear connection was seen between the four sciences of electricity, magnetism, heat, and light. The physical distinction between voltage and current, and the laws of combination of currents in parallel, and of voltages in series, were clearly recognized before exact measuring instruments existed, before the principles of energy were known, and before electrical units had been settled. When the conservation of energy was established, and when units of current and voltage were settled on energy principles so as to be independent of the properties of any special form of matter, it was seen that the law of the steady current circuit was merely an energy equation. The voltages V_1, V_2, etc., on the successive segments arranged in series, add up to V, the voltage of the generator, giving out a current I; thus

$$V = V_1 + V_2 + \ldots$$

because

$$IV = IV_1 + IV_2 + \ldots$$

i.e. the power output IV of the generator is equal to the sum of the amounts of power spent in the segments of the series circuit. A similar statement with V and I interchanged applies to the law of combination of currents in parallel.

It was thus pointed out long ago that the law of the simple circuit merely represents energy distribution, yet little note has been taken of the fact that in every case the relation between V and I is wholly experimental. Each resistance, or arrangement of matter, has its own volt-ampere characteristic. Even with a metal resistance there is no firm theory to explain the experimental fact that the ratio $V : I$ is the same, or nearly the same, for all currents. In the case of a motor with complicated and adjustable accessories in regard to winding, load, etc., although the engineer can predict the relation between V and I under specified conditions, the theory he uses is dependent on reasoning based on the law of the simple circuit, and this, except for energy considerations, is quite devoid of theory. Whether the magnetic, or the electric, circuit is in question the engineer merely selects from known experimental results those which he needs, and assembles them in a way to suit his purpose.

The law of the simple circuit is a framework based on energy, filled in with experimental data appropriate to the particular case. For this reason it is simple, universally applicable, and final. Nothing can upset it, since it is wholly experimental. A new theory may reinterpret, but can never alter, a single tested result.

Now the mathematical theory of current circuits must be based on the experimental facts, and in essence must be a statement of these facts in mathematical language. It cannot involve any theory of what is happening in the matter. Any such action has already been taken into account in the form of experimental characteristics relating to the successive portions of the circuit. This is true even of the Poynting flux, because this flux is based on mathematical theories consistent with tests on the circuit. However suggestive this flux may be as regards the mode of propagation of energy through the dielectric, it can reveal nothing new about the distribution of energy to the matter, since such distribution has already been assumed. The flux must be independent of any process by means of which the matter absorbs the energy and converts it to other forms.

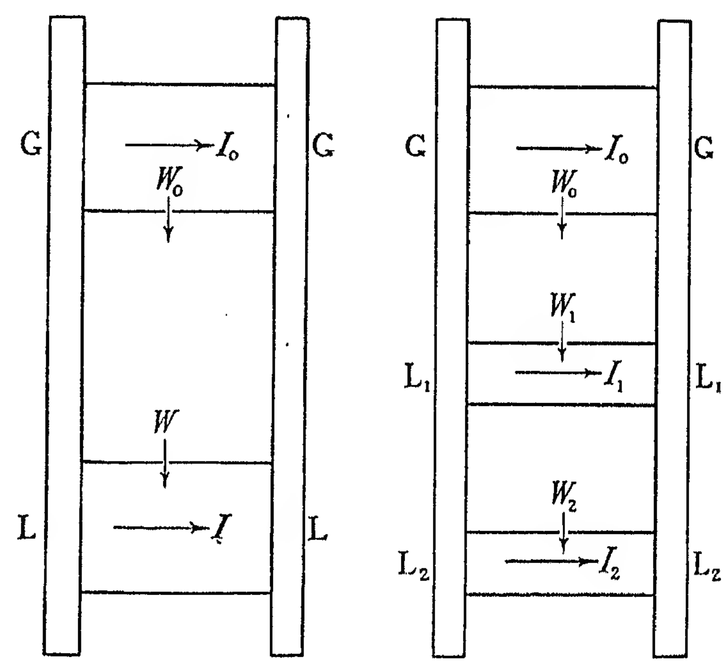

Fig. 8.

It is concerned solely with the net amount of energy delivered.

Now let us assume that we can determine W, the power entering a segment of the circuit, and the corresponding voltage V between the ends of the segment; but let us also suppose that we have no means of finding the corresponding current I. We can still define the current as a purely mathematical quantity measured by the ratio of W to V, and we can specify its direction as that of voltage-rise if W is emitted from the segment acting as a generator, and as that of voltage-fall if W is received by the segment acting as a load. All the ordinary laws of current combination follow, and in particular the law of circuital currents.

Thus if between two mains at voltage V we have (see Fig. 8) a generator G and a load L, and if the watts taken in are W_0 for G and W for L, we have $I_0 = W_0/V$, $I_1 = W/V$. No energy is lost or gained, and W_0 for the generator is negative. We have thus $W_0 + W = 0$ and $I_0 + I = 0$, or the current I flows in a closed circuit through G and L.

If the load consists of two parts L_1 and L_2 in parallel

we have similarly $W_0 + W_1 + W_2 = 0$, and $I_0 + I_1 + I_2 = 0$, or the currents form two circulating systems, one of I_1 through L_1 and G, the other of I_2 through L_2 and G.

These results follow from the definition of the current, and from the experimental characteristics connecting W with V for each of the current-carrying segments. They do not depend upon the nature of the segment, whether it is acting as a generator or as a load, or whether it does or does not contain matter. The sole assumption is that it is taking in or emitting electromagnetic energy. They must hold if one of the segments is a block of the dielectric, since, by Maxwell's theory, such a segment can contain electromagnetic energy, the amount of which can be altered. Poynting's theorem showed how the energy moved, and his interpretation of Faraday tubes gave a physical conception of the process. The Poynting flux is perpendicular to the electric force, or direction of greatest rate of change of voltage, while the current, as above defined, is parallel to this direction. The electrical current is thus always at right angles to the direction of energy flow.

Poynting's measure of a current I in a conductor is the rate of entry into it of electric tubes. Each tube as it leaves the generator, of voltage V, must contain an amount of energy denoted by V since the generator out-

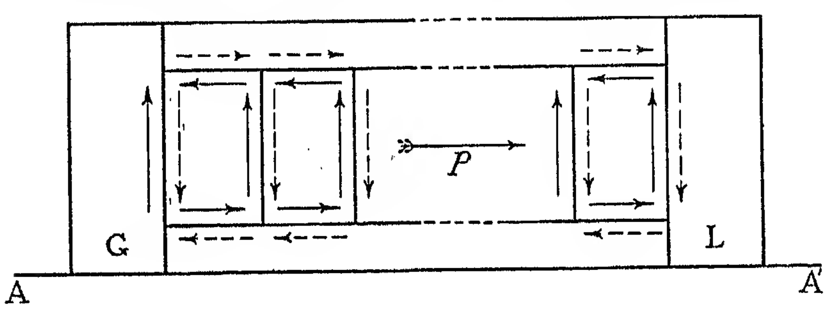

FIG. 9.

put is IV. As the tube progresses towards the load part of its energy is given up to the conductor, this portion being fixed by the corresponding drop of voltage in the conductor. If we refer again to the concentric cable denoted by Fig. 3 we can imagine the dielectric between the generator G and the load L to be divided up into blocks as shown by the rectangles in Fig. 9. Each arrow (except that denoting the flux **P**) shows the direction of the local current, and is drawn as a full line for a generator, or power output, current, and as a dotted line for a load, or power input, current. All the arrows represent currents of the same strength. Each current gives the ratio of watts to volts for the corresponding volume of the dielectric, or matter, and since the watts for successive blocks fall off in the same proportion as the volts, the ratio of the two, or the current, is the same in all cases.

The arrows also illustrate the movement of Faraday tubes as imagined by Poynting. Under steady current conditions the arrows in successive blocks can be looked upon as showing either the successive positions of the same tube or the positions at the same instant of tubes forming a sequence.

There are equal and opposite currents on each side of a surface separating two blocks of the dielectric, or separating a region containing matter from another region devoid of matter. We can look upon Fig. 9 as showing a conduction current going round the material circuit in a closed path, in addition to a number of dielectric currents each circulating round a dielectric block in the opposite sense to that of the conduction current. It is, however, possible to regard these circulating currents as more local in character, as indicated in Figs. 10 and 11, in which each arrow representing an emission current is associated with the arrow denoting the corresponding absorption current. The current circuits that are confined to the dielectric indicate merely the passage of energy, without change in amount or transformation in kind. Under steady conditions these

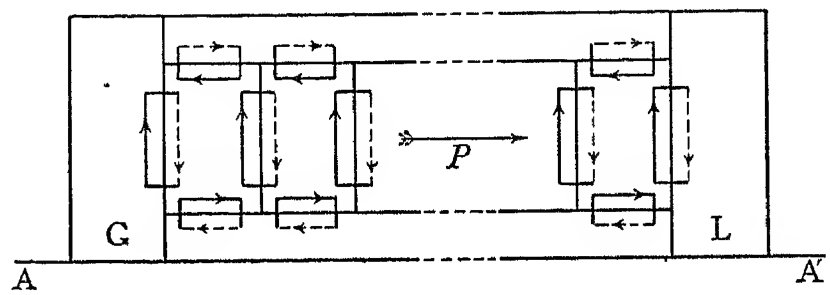

FIG. 10.

circuits must neutralize each other and can be omitted from Fig. 11. On the contrary the current circuits through regions partly occupied by matter denote energy which is not only transferred but also transformed. Under steady conditions the rate of absorption, or emission, of this electromagnetic energy is constant. We can suppose that the dielectric blocks are indefinitely thin, so that the circuits in Fig. 11 can be regarded as very minute. In all cases the current flows in a closed circuit. Maxwell's principle of closed current circuits merely expresses, in electrical language, an aspect of the conservation of energy.

The transfer of energy from tube to matter, or vice versa, appears to be a strictly local process due to the

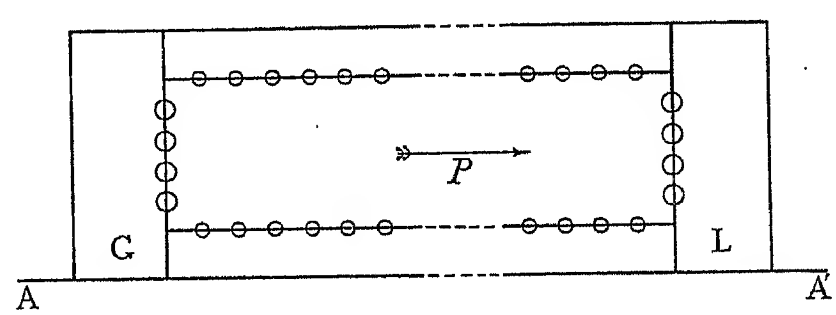

FIG. 11.

impulsive closure of an element of the tube round a minute and isolated particle of the matter. Theories of matter, however they may differ in other ways, seem all to agree on two points: (i) that the ultimate particles of matter are excessively small compared with the distances which separate them, and (ii) that the transfer of energy to or from matter is of an impulsive character. The possibility of direct action at a distance is denied because it is not admitted that a physical influence or process can progress through space with an indefinitely infinite velocity. If the transfer of energy to or from a material particle is impulsively rapid, it must also be strictly local, and since the particle is isolated and

excessively small, the transfer process must be confined to the immediate neighbourhood of the corresponding point. The local-impulse assumption, used in the analysis forming the second part of the present paper, seems to be only a form of the principle of local action.

In the case illustrated in Figs. 9 to 11 it is assumed that the flux system is single. The processes at work must be the same if the system is multiple. Any tubes reflected after incidence on matter are emitted from it as if from an additional generator. There are a number of superposed systems each like Fig. 9, but the compound state must settle down to one of the type shown in Fig. 11.

Current density is measured by the rate at which electric tubes enter the unit of cross-area. It is $\dot{B}_e$ both in the dielectric and in matter, but while the mathematical measure is the same the physical meaning is different. In the dielectric the tubes persist, but in matter the energy of the tube may be given up to the matter acting as a load, or emitted from the matter acting as a generator. In one case the tubes are destroyed, and in the other they are created. In a steady state the generator creates the tubes as fast as it emits them, while the load destroys the tubes as fast as it receives them.

ELECTROMAGNETIC FORCES SET UP BETWEEN CURRENT-CARRYING CONDUCTORS DURING SHORT-CIRCUIT.*

By G. L. E. METZ, Associate Member.

(*Paper received* 3*rd October*, 1933.)

SUMMARY.

This paper deals in a general manner with the electromagnetic forces exerted between current-carrying conductors. The factors introduced by the use of alternating current are taken into account and the effects upon the forces of conductor shape, current distribution, and proximity of inductive material, are considered. The characteristics of the forces, the reactions they produce, and the effects of resonance, are also examined.

The results of the investigation are given either in general terms or in the form of equations, which are presented in a manner convenient for use in the calculation of electromagnetic forces on alternating-current circuits.

INTRODUCTION.

The theory underlying the calculation of electromagnetic forces has received very thorough academic treatment, most notably by Gray† and, later, by Hague,‡ who clearly define the limitations of the basic formulæ enunciated by the early investigators.

The application of these formulæ to the somewhat special problems which arise under short-circuit conditions on a.c. networks has not been so rigorously pursued, with the result that the further limitations set by practical considerations are not generally known.

This is particularly true of many of the problems presented to switchgear engineers. Among these the calculation of the electromagnetic forces exerted upon busbars, connections, and oil circuit-breaker cross-bars, terminal stems, and contact fingers, are common examples. Considerable confusion surrounds the precise interpretation of calculated results for problems of this type, owing to the effects of the value, phase order, and state of balance, of the short-circuit current used in the calculations; and also to the effects of current distribution in the conductors, skin and proximity effects, presence of inductive material, and the pulsating character of the applied forces, all of which are known to influence the reactions produced.

It is the object of this work to form a corollary dealing with the application of the known methods of calculation to practical cases of straight conductors under short-circuit conditions on a.c. networks, taking into account the modifications produced by these influencing factors.

Where possible, a mathematical investigation of the various factors is made; in other cases a guide to their influence upon the forces is obtained from an examination of the underlying principles involved, augmented where possible by actual tests.

The well-known d.c. formulæ are used to calculate the forces between various arrangements of conductors, and the limitations set by the distribution of the current in the conductors, the proximity of inductive material, and the effect of practical a.c. network conditions, are considered. In each instance the general theory underlying the phenomena is briefly enunciated and precise calculations, or test results of particular cases, are given.

(1) REVIEW OF KNOWN METHODS OF CALCULATION.

The calculation of electromagnetic forces may be effected by two distinct methods. The first is particularly applicable to straight conductors, and the second —not so often used—is especially applicable to arrangements of conductors of which the self-inductance and mutual inductance are known.

To enable the application of these two methods to be appreciated, the assumptions upon which they are based will now be briefly reviewed.

Method 1.

The formulæ most generally used for the calculation of electromagnetic forces are based upon the experiments of Ampère, which formulated the laws of mechanical interaction between electric currents. In these experiments the intensity of the magnetic field produced by a current-carrying conductor was established and defined in the Laplace law, which states: "The force exerted upon a magnetic pole by an infinitely short element of a current-carrying conductor is inversely proportional to the square of the distance and directly proportional to the length of the element, to the current strength, to the pole strength, to the permeability of the surrounding medium, and to the sine of the angle of inclination of the element to the line joining the pole to the centre of the element."

It follows that the intensity of the magnetic field H at a point P (Fig. 1) due to the current i flowing in the element dl when placed in a medium of permeability μ is given by

$$H = (\mu i/s) \cos \theta d\theta$$

The total field H_t at P, due to the current in the whole of conductor AB, obtained by integrating this expression between the limits θ_1 and θ_2, is given by

$$H_t = (\mu i/s)(\sin \theta_1 + \sin \theta_2) \qquad (1)$$

The direction of this field is at right angles to the current, as shown in the figure.

* Abstract of a thesis accepted in lieu of the Graduateship Examination.

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† See Bibliography, (1). ‡ *Ibid.*, (2).

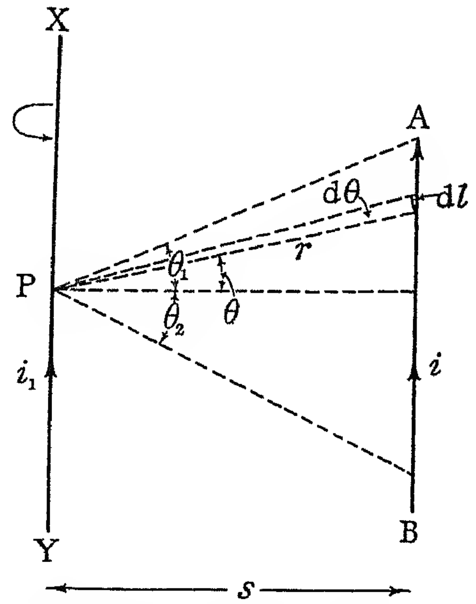


FIG. 1.—Intensity of magnetic field produced by a current-carrying conductor.

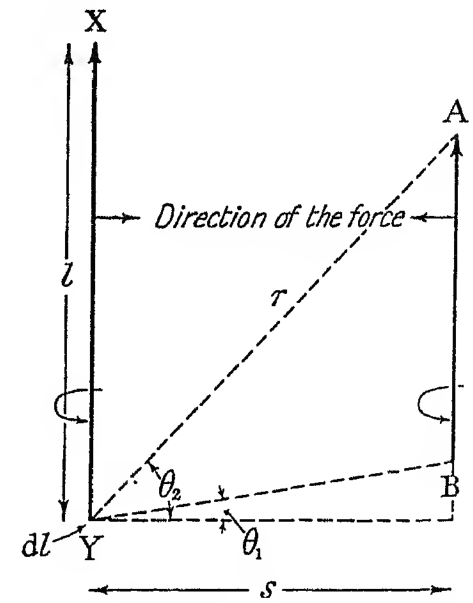


FIG. 2.—Force exerted between current-carrying conductors.

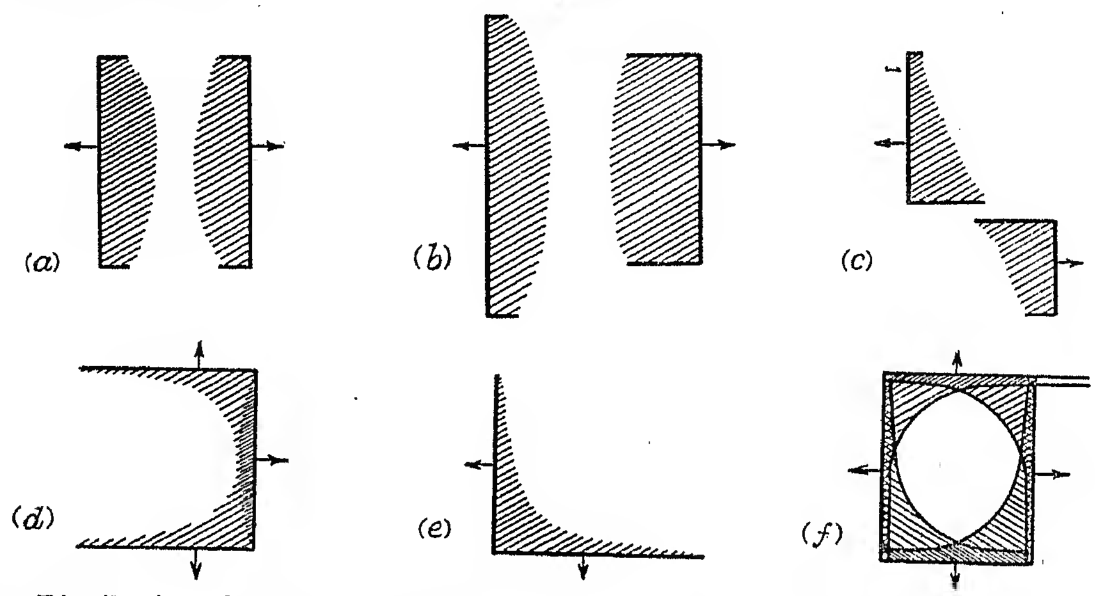


FIG. 3.—Distribution of electromagnetic force on various arrangements of straight conductors.
(a) Parallel conductors of equal length. (b) Parallel conductors of unequal length. (c) Parallel conductors in different planes. (d) Conductors arranged in a loop. (e) Conductors at right angles. (f) Conductors arranged in a rectangle.

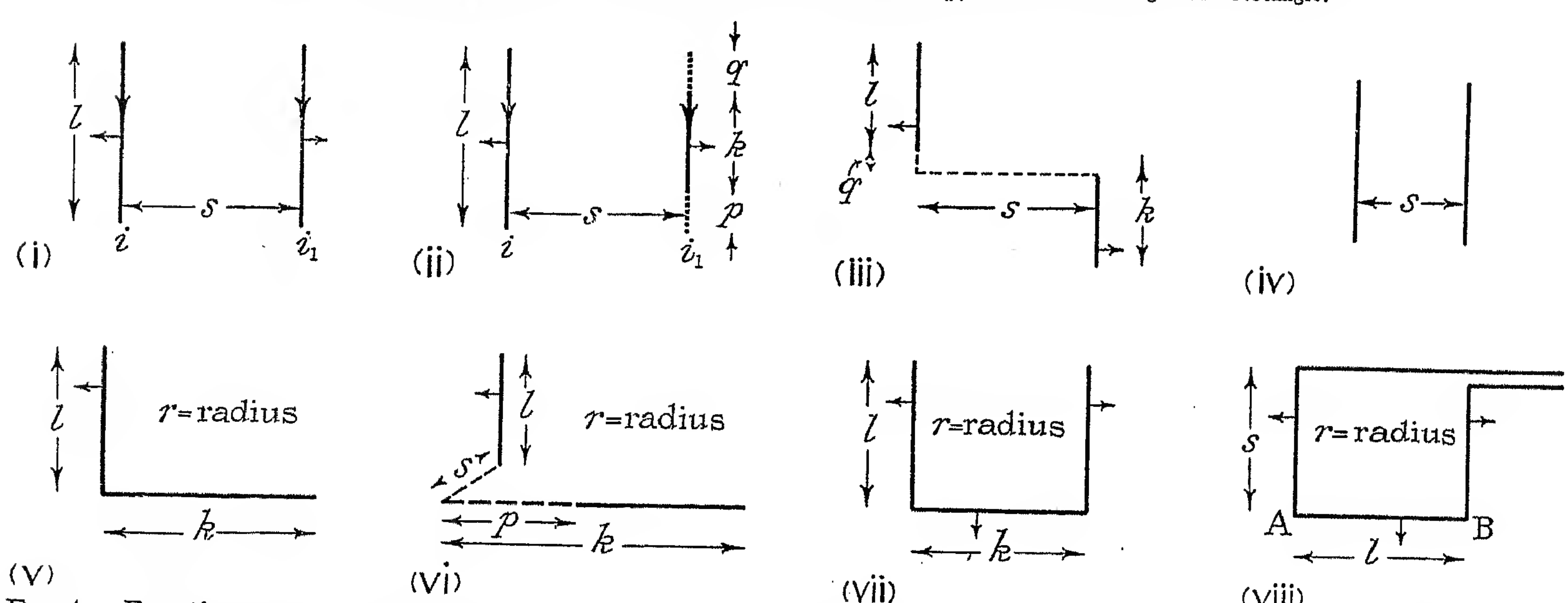


FIG. 4.—Equations giving electromagnetic force on various arrangements of straight conductors; assuming (a) current in amps. (d.c.), (b) current concentrated on centre lines, (c) conductors located in medium whose permeability is 1.

(i) $F=\frac{4\cdot 5ii_1}{10^8 s}\sqrt{(l^2+s^2-s)}$ lb.

(ii) $F=\frac{2\cdot 25ii_1}{10^8 s}\left\{\sqrt{[(k+p)^2+s^2]}-\sqrt{(p^2+s^2)}-\sqrt{(q^2+s^2)}+\sqrt{[(l-p)^2+s^2]}\right\}$ lb.

(iii) $F=\frac{2\cdot 25ii_1}{10^8 s}[\sqrt{(l^2+k^2+s^2)}-\sqrt{(l^2+q^2+s^2)}-\sqrt{(k^2+s^2)}+\sqrt{(q^2+s^2)}]$ lb.

(iv) $F=\frac{5\cdot 4I^2}{s}\times 10^7$ lb. per ft., where s is in inches (for long parallel conductors).

(v) $F=\frac{2\cdot 25ii_1}{10^8}\left[\log\frac{2k}{r}-\log\frac{k+\sqrt{(k^2+l^2)}}{l}\right]$ lb.

(vi) $F=\frac{2\cdot 25ii_1}{10^8}\left[\log\frac{\sqrt{(k^2+l^2+s^2)}-k}{\sqrt{(p^2+l^2+s^2)}-p}-\log\frac{\sqrt{(k^2+s^2+r^2)}-k}{\sqrt{(p^2+s^2+r^2)}-p}\right]$ lb.

(vii) $F=\frac{4\cdot 5ii_1}{10^8}\left\{\log\frac{2k}{r}-\log\left[\frac{k+\sqrt{(k^2+l^2)}}{l}\right]\right\}$ lb.

(viii) $F=\frac{4\cdot 5ii_1}{10^8}\left[\log\left\{\frac{(2ls)}{r(l+d)}\right\}+\frac{d}{s}-1\right]$ lb.

By another fundamental law of magnetism the force exerted upon a current-carrying conductor when placed in a magnetic field is proportional to the conductor length, the current flowing in it, the intensity of the magnetic field, and the sine of the angle made by the directions of the current and of the flux cutting it. The force F exerted in air upon the element dl (Fig. 2) carrying a current i_1, when placed in a field of intensity H_i, is therefore given by

$$F = (ii_1/s)(\sin\theta_2 - \sin\theta_1)dl$$

and the total force F_t upon the whole of conductor XY, due to the field produced by the current in AB, is obtained by integrating this expression between suitable limits. The direction of this force is at right angles to that of the current.

It follows from equation (1) that the magnetic intensity, and therefore the electromagnetic force, exerted at any point P in conductor XY due to the current in conductor AB, depends entirely upon the values of θ_1 and θ_2. The force actually varies between the limiting values $2\mu ii_1/s$ when P is taken in a mid-position and θ_1 and θ_2 approach 90°, and $\mu ii_1/s$ when P is taken at one end of XY such that θ_1 and θ_2 approach 90° and 0° respectively.

The distribution of force along conductors for various conductor arrangements has been calculated and is shown in Fig. 3. It is important when calculating electromagnetic forces to state whether the results refer to the force at a point, the total force, or the average force, and if the latter value is used it is necessary to observe that the maximum force will be considerably greater than the average value according to the conductor arrangement. In cases where lever action is encountered it is necessary to take "moments" to allow for this uneven loading. The Ampère method has been used to calculate the forces for the arrangements of conductors generally used, and the final equations are given in Fig. 4.

Method 2.

Faraday's law of induction established the fact that if the magnetic flux linked with a circuit is changed by moving the circuit relative to another circuit or to inductive material, or by altering its shape, work is done in overcoming the electromagnetic forces opposing the motion and corresponding adjustments occur to the electrical power supplied to the circuit. It can be shown that the mechanical work done in changing the coefficient of inductance plus the change in the energy stored magnetically in the circuit equals the change in the electrical energy supplied to the circuits.

If F is the force required to move or deform the circuit shown in Fig. 5 through a distance dy, then

$$F = \tfrac{1}{2}i_1^2\frac{dL_1}{dy} + \tfrac{1}{2}i_2^2\frac{dL_2}{dy} + i_1i_2\frac{dM}{dy} \quad . \quad . \quad (2)$$

When the circuits are not in close proximity to inductive material and are unchanged in shape, the self-inductances L_1 and L_2 remain unaltered when the circuits are moved relative to one another, and the formula simplifies to

$$F = i_1i_2dM/dy \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This relation can be used to calculate the electromagnetic forces produced between arrangements of conductors the self-inductance and mutual inductance of which are known.

If the mutual inductance between the two parallel conductors shown in Fig. 4(i) be given by

$$M = 2\left[l\log\frac{l^2 + \sqrt{(l^2+s^2)}}{s} - \sqrt{(l^2+s^2)} + s\right]$$

(l and s being measured in centimetres), the electromagnetic force between them can be calculated by means of equation (3). The value obtained will be found to be identical with that given in Fig. 4(i), which was calculated by Ampère's method.

The case of conductors meeting at right angles, which is encountered when considering the throw-off forces on oil circuit-breaker cross-bars and isolating switches, is somewhat special and merits consideration.

Conductors Meeting at Right Angles.

If, in Fig. 6(a), it is assumed that the current is concentrated upon the centre lines of the conductor, the downward force on AB due to the current traversing the loop can be calculated by integrating the force

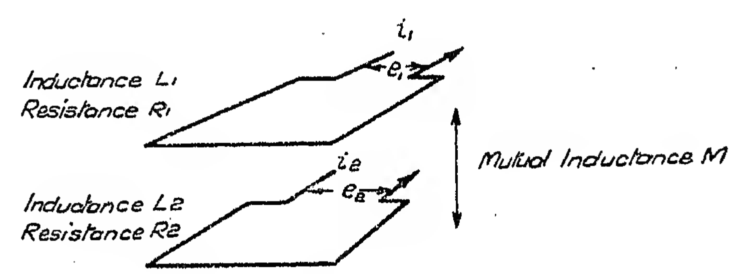


FIG. 5.

exerted by one current element upon another. It can be shown that the downward force on any point P in AB is given by

$$F = 2i^2l/(sr) \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This force increases slowly from a minimum value in the middle of AB to rise steeply to infinity at each end at the junction of the centre lines of AB and the vertical conductors, as shown in Fig. 3(d). Actually the current is not concentrated upon the centre lines of the conductors, and cannot make a sharp right-angle change in direction; therefore these infinite values of force are not encountered in practice.

If, in Fig. 6(b), the integration of the throw-off force along AB is taken over the length s_1 only, the following expression is obtained for the total throw-off force in dynes:—

$$F_t = 2i^2\left[\log\frac{2s_1}{r} - \log\frac{s_1 + \sqrt{(s_1^2 + l^2)}}{l}\right] \quad . \quad (5)$$

The results obtained by this formula check closely with those obtained by test upon a system of conductors built up as shown in Fig. 6, in which the section of the conductors is small and the current distribution approximately uniform.

It would appear from these tests that the current distribution in the conductor is such as to prevent the extreme corner effects being experienced in practice,

since calculations on the lines indicated agree very closely with actual test results.

It does not follow that the throw-off forces upon an oil circuit-breaker cross-bar, which is an example of this type, can be calculated by this means with the same degree of accuracy. Generally the size and disposition of the conductors introduce skin and proximity effects, which, together with the proximity of an inductive top plate and tank, affect the values of the electromagnetic forces exerted.

The considerations discussed so far are based upon the following assumptions: (i) That the current is concentrated upon the centre lines of the conductors, or that the current is uniformly distributed over the conductors, which are circular in shape. (ii) That the conductors are located in a medium such as air, the permeability of which is unity, and are not in close proximity to inductive material. (iii) That direct current is flowing in the conductors. When the conductors are spaced a considerable distance from one another, as in the case of two parallel busbars, or when the conductors are small

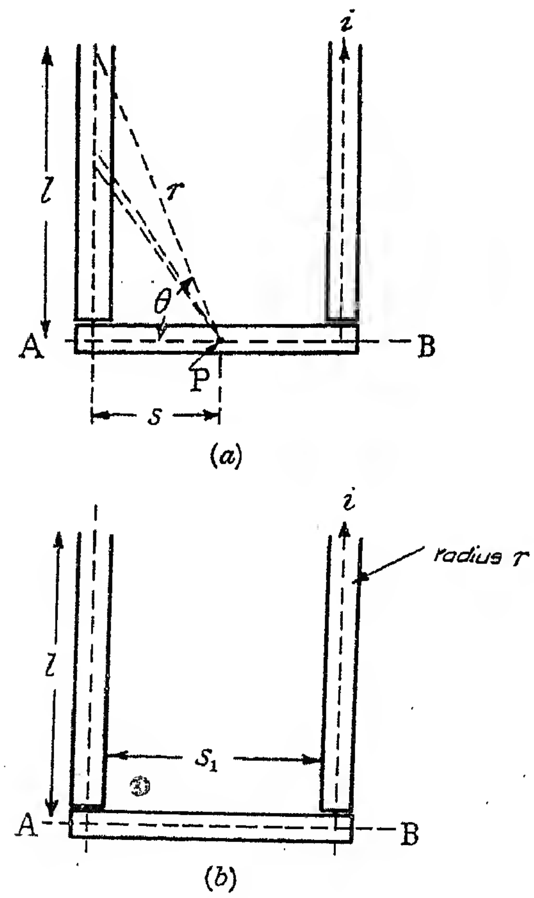


FIG. 6.—Total force exerted upon conductors AB.

in section, the actual shape of the conductors makes very little difference to the calculated results.

In practice the conductors are generally of considerable section, and in many cases are located close to one another; it then becomes necessary to modify the formulæ so as to take the shape of the conductors into account.

Effect of Conductor Shape.

The effects produced on the forces when rectangular conductors are used instead of conductors of circular or square section have been investigated mathematically by Dwight,* who has shown that the force in dynes per centimetre exerted between two long parallel rectangular conductors of depth $2b$ and thickness $2a$, placed a distance d apart, is given by

$$\begin{aligned}F=\frac{i_1 i_2}{16a^2b^2}\Big\{&4b\Big[(d+2a)^2-\frac{4b^2}{3}\Big]\arctan\frac{2b}{(d+2a)}\\&+4b\Big[(d-2a)^2-\frac{4b^2}{3}\Big]\arctan\frac{2b}{(d-2a)}\\&-8b\Big(d^2-\frac{4b^2}{3}\Big)\arctan\frac{2b}{d}-2d\Big(4b^2-\frac{d^2}{3}\Big)\log\Big(\frac{d^2+4b^2}{d^2}\Big)\\&+(d+2a)\Big[4b^2-\frac{(d+2a)^2}{3}\Big]\log\Big[\frac{(d+2a)^2+4b^2}{d^2}\Big]\\&+(d-2a)\Big[4b^2-\frac{(d-2a)^2}{3}\Big]\log\Big[\frac{(d-2a)^2+4b^2}{d^2}\Big]\\&+\frac{2}{3}(d+2a)^3\log\Big(\frac{d+2a}{d}\Big)+\frac{2}{3}(d-2a)^3\log\Big(\frac{d-2a}{d}\Big)\Big\}\end{aligned}$$

The force exerted in air between the conductors shown in Fig. 4(iv), assuming the current to be concentrated

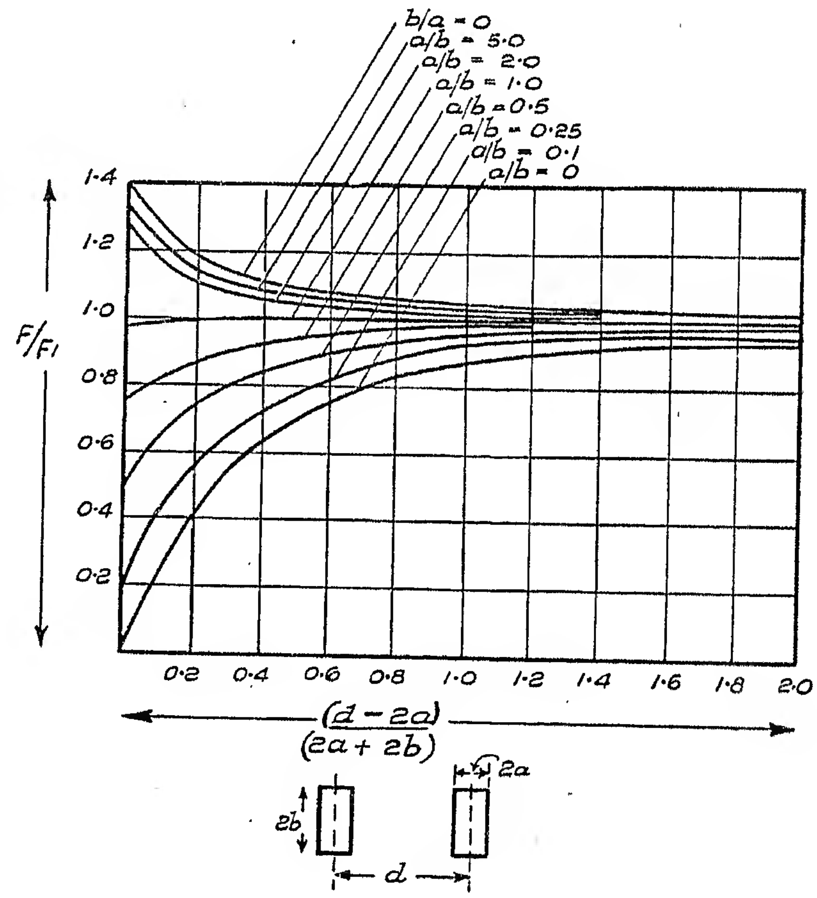


FIG. 7.

upon the centre lines, has already been shown to be given by $F_1 = 2i_1i_2/d$ dynes per cm. The ratio F/F_1 gives a shape factor which indicates the effect produced on the forces if conductors of rectangular section are used.

In Fig. 7 this ratio has been plotted for the sizes of rectangular conductors generally used for switchgear busbars and connections. It is shown in this figure that for conductors of square section the assumption that the current is concentrated upon the centre lines makes very little difference to the calculated results. The curves in Fig. 7 are used in the following manner. First the force F_1 is calculated, assuming the current to be concentrated upon the centre lines of the conductor

* See Bibliography, (3).

and using the formula $F_1 = 2i_1i_2/d$ dynes per cm $= (5 \cdot 4i_1i_2/d) \times 10^{-7}$ lb. per ft., where the current is measured in amps. and d is in inches. From the known values of a and b the value of $(d - 2a)/(2a + 2b)$ is next obtained, and the corresponding figure for the ratio F/F_1 is read off from the appropriate curve.

The maximum effect occurs when single strap connections of negligible thickness are used. In the extreme cases taken in the figure the force is increased by 40 per cent in the maximum case, and reduced to 20 per cent of the value obtained by assuming the current to be concentrated upon the centre lines in the minimum case.

It is also shown that the effect of conductor shape upon the value of the calculated force decreases very rapidly as the spacing of the conductor increases. When the spacing between conductors is greater than half the sum of the sides of a single conductor the forces are not affected by more than 10 per cent, and as the spacing increases the effect rapidly becomes negligible.

The formula upon which these curves are based compares closely with the experimental results obtained by Barrow* for direct current where the current distribution is approximately uniform.

When the conductors carry alternating current the current distribution may be very far from uniform, and the forces are accordingly modified.

It is not possible to deal with the effects produced by non-uniform current distribution in general terms, but the application of certain underlying principles reveals many important effects which are not generally known. It indicates that generally where the force is one of attraction it will tend to decrease and when the force is one of repulsion it will tend to increase.

(2) Skin and Proximity Effects.

General.

The distribution of alternating current in a conductor is controlled by the disposition of the magnetic fields and by the e.m.f.'s they produce in the elementary filaments of the conductor. The variation of the e.m.f.'s induced in the elementary filaments of the conductors due to the internal fields of the elements themselves results in a tendency for the current to flow in the outer filaments of the conductor and also for the current in the outer filaments of the conductor to advance in phase upon that in the inner filaments. This phenomenon is known as " skin effect."

The variation of the e.m.f.'s induced in the filament of the conductor due to the external field of neighbouring conductors results in a tendency for the current to crowd to one side of the conductor. This phenomenon is generally known as " proximity effect," and although a factor of great importance in all electromagnetic problems it has so far been largely ignored.

The phenomenon is illustrated in Fig. 8(a), which shows the field associated with a single-phase lead and return current flowing down into the paper in conductor X and returning up through the paper along conductor Y. The flux from conductor X cuts the elementary filaments of conductor Y, inducing in them an e.m.f. which causes the current to circulate down on the right-hand side of Y and up on the left-hand side. This results in the current crowding on to the sides of the conductors adjacent to one another.

If the current flows down into the paper in conductors X and Y, as shown in Fig. 8(b), the current in these conductors is by the same reasoning crowded on to the sides of the conductors remote from one another. This effect can be memorized from the simple rule that the magnetic field set up between two current-carrying conductors which results in a force tending to move the conductors in one direction, also tends to cause the current in the conductors to move in the opposite direction.

These factors have an important effect upon the forces produced between conductors of comparatively large section located close to one another, such as the finger contacts of oil switches. When the force is one of attraction the currents tend to move across the conductors and away from one another, thus tending to decrease the force. On the other hand, repulsive forces tend to cause the currents to move closer to one another, with a consequent increase in force. The general effect is most undesirable upon contacts which depend for their efficient operation on good contact pressure, as will be explained in greater detail later.

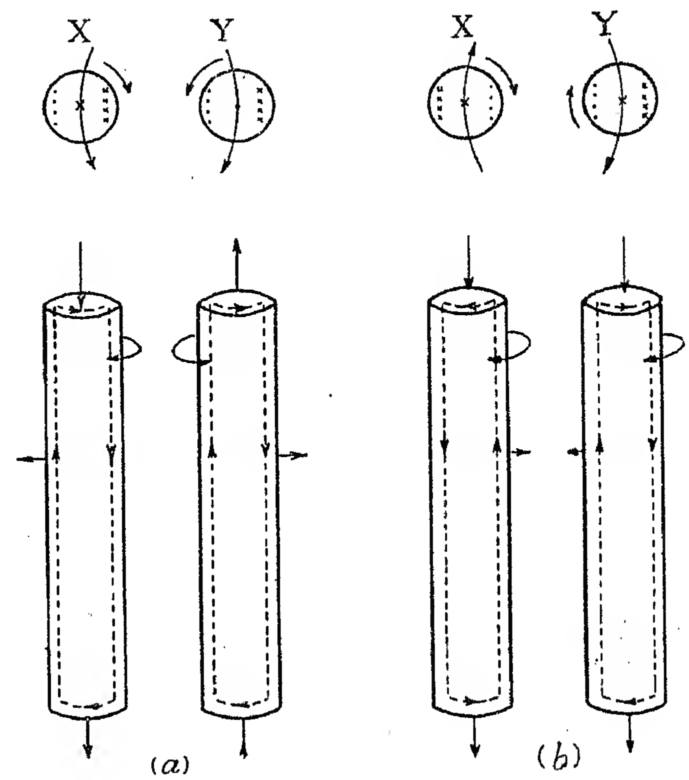


Fig. 8.

The manner in which the current would be expected to flow in the loop of an oil circuit-breaker owing to skin and proximity effects is shown in Fig. 9, from which it will be seen that the current tends to flow upon the inside of the loop.

The peculiar burning of " butt type " contacts which appears after interrupting short-circuit currents gives ample evidence that the current does in fact flow round the loop formed by the oil switch in the manner indicated.

Further forces are also exerted between switch contacts by the so-called " crowd effect," which occurs whenever current passes from one contact to another.

* See Bibliography, (4).

Crowd Effect.

Since it is impossible in practice to prepare an infinitely smooth surface, it follows that the electrical contact between two contact surfaces can at best only be made at a number of points of very small area. An alternating current in passing from one contact to another must therefore cross the interface at a number of point contacts. The current in the two conductors converges toward the local points of contact at the interface, and under the action of the skin effect this flow takes place in the skin of the metal next to the dividing surface. This results in the flow of currents closely adjacent and opposite in direction to one another in the skin of the two contact surfaces, producing repulsive forces.

As the distance separating the currents at the interface is very small the resulting force of repulsion is correspondingly large, and the contacts of oil switches have been known to be forced apart by this effect when large short-circuit currents have been flowing. An example of this phenomenon is shown for an extreme case in Fig. 10, which illustrates the approximate current distribution between two conductors making contact on one edge only.

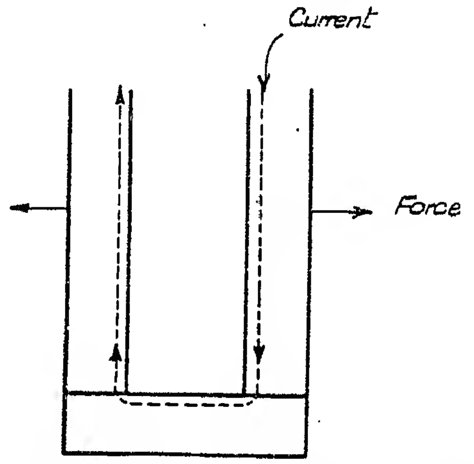


FIG. 9.—Passage of current round loop of an oil circuit-breaker. (Current flows on inside of conductor.)

The effect of the current distribution in the contacts is to produce a force tending to separate the contact surfaces in the manner shown. According to Clerc* this force may be calculated by means of the formula

$$F = 3 \cdot 5 I^2 \log_{10}(60/D) 10^{-8}$$

where D = diameter of actual contact in mm^2, I = peak value of the first half-cycle, including doubling effect in amps., and F = force in lb. The practical value of this formula is restricted, since the diameter of the actual area of contact is never known. By assigning various values to D in the equation, however, it is possible to get an idea of the order of the force involved.

In general the effects do not lend themselves to precise calculation, but this brief study of the underlying phenomena provides a useful guide in design which enables these undesirable characteristics to be avoided.

Proximity of Inductive Material.

The presence of inductive material increases the value of the permeability from unity (in air) to some greater value. The reluctance of the magnetic circuit is as a result decreased and redistributed. The magnetic field produced by a given current is consequently modified, resulting in a change and redistribution of the forces produced.

* See Bibliography, (5).

These effects do not lend themselves to a general mathematical treatment, but particular cases can be experimentally investigated comparatively easily by the change-in-inductance method. Considerable guidance in regard to design can be obtained from a consideration of the general effect of the presence of inductive material upon the magnitude and distribution of flux produced by a given current, since upon this the ultimate forces depend. For this purpose the Theory of Images* is most useful. By this theory it can be shown that the effect produced upon a current-carrying conductor owing to the presence of a screen of infinitely permeable material can be reproduced by replacing the screen by a current-carrying conductor similar in every respect to the original conductor and placed as far behind the screen as the latter is in front. If the permeability of the inductive screen is finite and equal to μ, then it can be shown that the effect of the screen can be reproduced by replacing it by an image conductor carrying a current $[\mu - 1/(\mu + 1)]i$ in the same direction as the current i in the original conductor. The calculation of the force upon a long straight wire close to a block of iron of permeability μ can then be made by means of the formula given in Fig. 4(i), putting $i_1 = [\mu - 1/(\mu + 1)]i$ and assuming the value of s to be twice the distance separating the conductor and the iron.

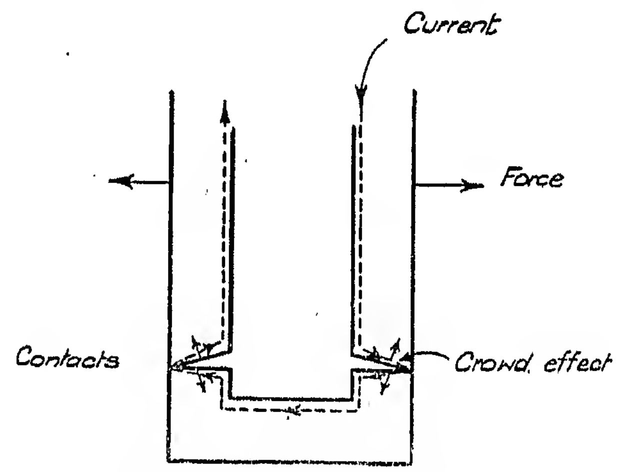


FIG. 10.—Forces set up between contact faces by crowd effect.

The general effect produced upon the flux distribution around two conductors carrying a lead and return current, owing to the presence of inductive screens placed in various positions, is shown in Fig. 11. This figure indicates that the presence of inductive material may decrease or increase the various components of force on the conductor according to the location of the former.

(3) NETWORK FACTORS.

Practical Considerations.

Before the d.c. formulæ given in Fig. 4 can be used to calculate the electromagnetic forces under short-circuit conditions it is necessary to define the value of

* See Bibliography, (2).

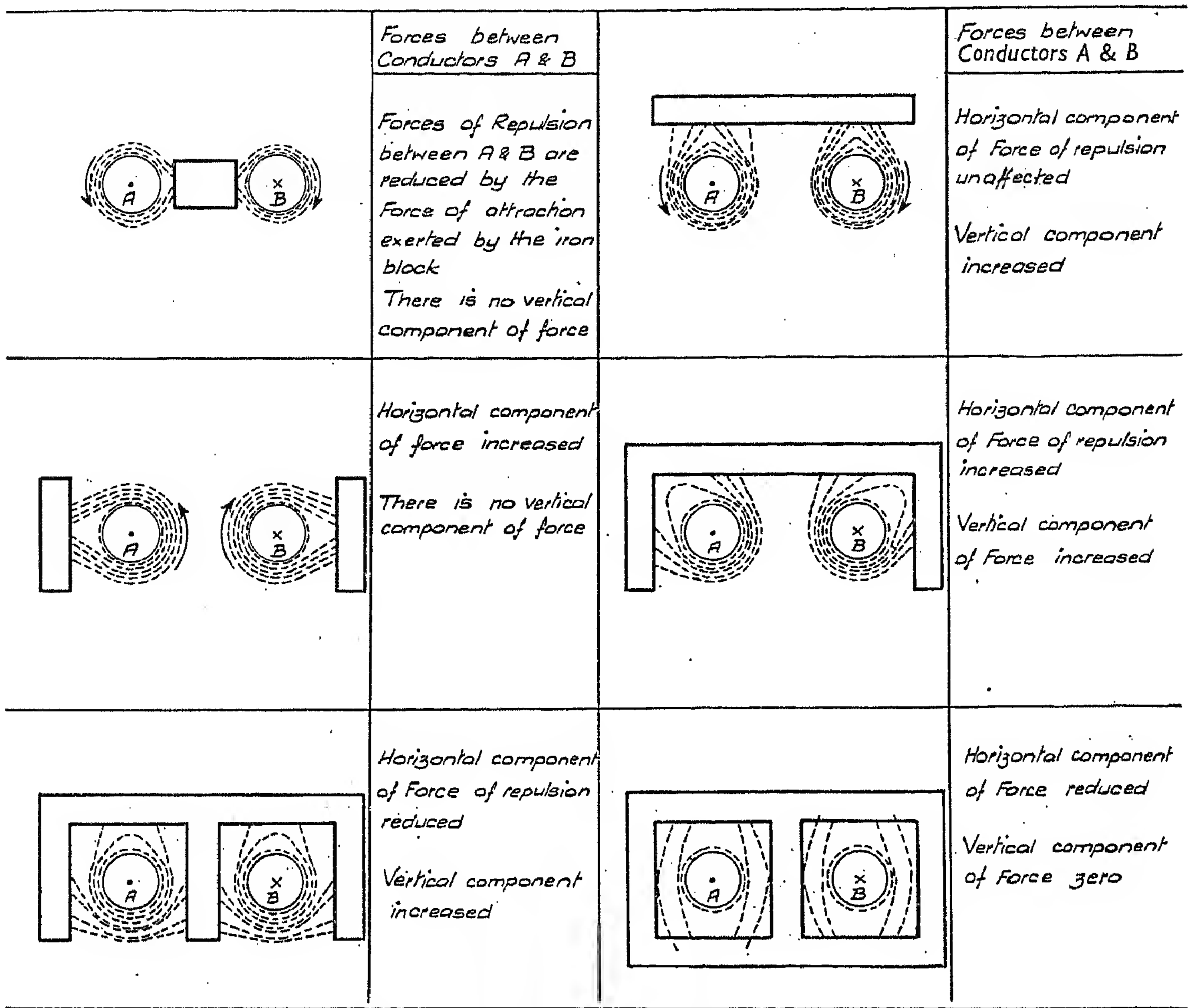


FIG. 11.—Effects produced on forces on conductors A and B due to the presence of inductive material.

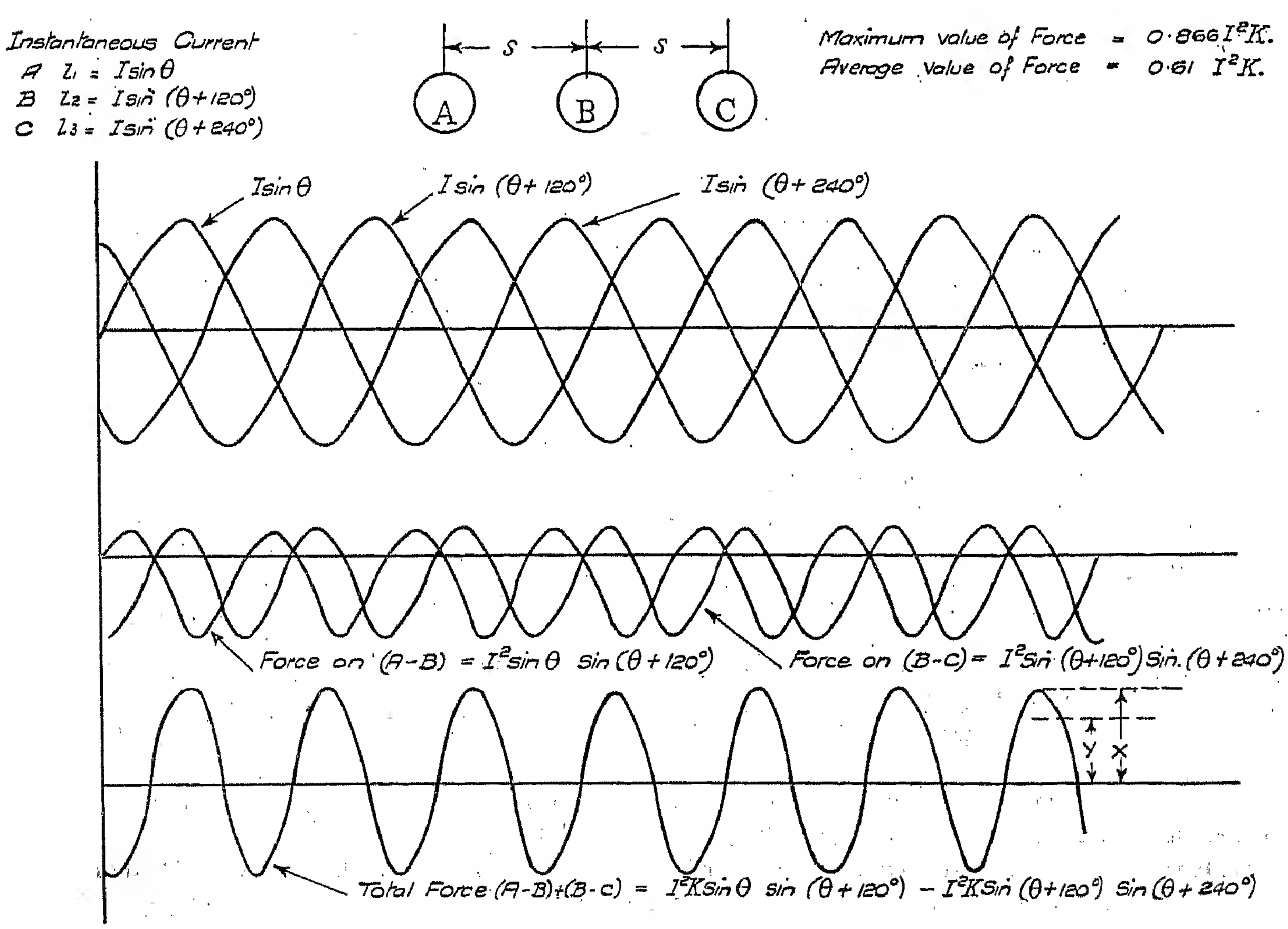


FIG. 12.—Force on middle conductor B due to a balanced 3-phase short-circuit, ignoring decrement.

the current to be substituted in them. The factors which control the value of the short-circuit current upon an a.c. network vary with almost every case, and it becomes necessary to make certain assumptions which may reasonably apply to the majority of cases.

Since all equipment has to be designed to withstand the maximum forces that can occur, this is the condition most likely to meet the majority of cases. The force is proportional to the square of the current, and the maximum force occurs with the application of the peak value of the first half-cycle of the short-circuit current, after which the current decreases owing to decrement factors. The peak value of the first half-cycle of the short-circuit current is controlled to a considerable extent by the effects of asymmetry.

TABLE 1.

Phase order	Conductor arrangement	Maximum asymmetry factor
Single	$\odot$ $\otimes$	$3 \cdot 24$
2-phase 3-wire (outer)	$\odot$ $\otimes$ $\odot$	$4 \cdot 56$
2-phase 3-wire (middle)	$\odot$ $\otimes$ $\odot$	$3 \cdot 24$
3-phase (outer)	$\odot$ $\otimes$ $\odot$	$2 \cdot 62$
3-phase (middle)	$\odot$ $\otimes$ $\odot$	$2 \cdot 81$

The Effects of Asymmetry.

It is generally agreed that the effects of complete asymmetry increase the peak value of the first half-cycle of the short-circuit current of a normal machine by $1 \cdot 8$ times its symmetrical short-circuit value. Complete asymmetry will therefore increase the peak value of the short-circuit current in each conductor of a single-phase system to $1 \cdot 8i$, and will increase the electromagnetic forces by $1 \cdot 8^2$ ($= 3 \cdot 24$) times the symmetrical value.

The maximum effect of asymmetry upon a 3-phase system is not, as is generally supposed, experienced when full asymmetry occurs on one line. The effect of asymmetry varies as the cosine of the angle of displacement of the voltage wave from its zero value. It has a maximum value when the angular displacement is zero ($\cos 0° = 1$), and a minimum value when the angular displacement is 90° ($\cos 90° = 0$). Considering the middle conductors of the 3-phase system shown in Fig. 12, the maximum value of asymmetry is therefore given by the maximum value of $[1 \cdot 8 \cos \theta \times 1 \cdot 8 \cos (\theta + 120°)] - [1 \cdot 8 \cos (\theta + 120°) \times 1 \cdot 8 \cos (\theta + 240°)]$, which is equal to $2 \cdot 81$.

It is not generally appreciated that in polyphase systems the total effects of asymmetry vary with the disposition of the conductors. The effect of asymmetry has been investigated for certain arrangements of conductors in single-phase and polyphase systems, and the results are given in Table 1 in the form of asymmetry factors. In calculating these factors it was found that in certain cases (e.g. the middle conductor of a 2-phase or 3-phase system) the maximum effects of asymmetry were coincident with the maximum value of the force. In other cases (e.g. the outer conductor of a 2-phase or 3-phase system) the maximum effects of asymmetry occurred when the force was at or near its zero value. In the latter cases, therefore, the effects of asymmetry can be completely ignored, since no matter how large they become the force wave still remains at its zero value.

The maximum forces (in lb. per ft. run) that can occur upon a.c. systems and the corresponding factors for substitution in the d.c. equations (Fig. 4) have been calculated for the types of systems in general use, and are given in Table 2. In the formulæ the following assumptions are made: (1) The most severe condition of asymmetry. (2) Balanced 2-phase or 3-phase short-circuits. (3) Conductors unaffected by presence of inductive material. (4) Current measured in r.m.s. amps. and s measured in inches.

Single-Phase and 3-Phase Electromagnetic Effects.

In Table 2 it is shown that the amplitude of the force produced by a single-phase short-circuit current is greater than that produced by a 3-phase short-circuit current of equal value. The single-phase case produces a pulsating force, and the 3-phase case produces a force upon the middle conductor which alternates from a maximum in one direction to a maximum in the other direction. To decide which of these conditions is likely to be the more severe it is necessary to determine which type of fault gives the greater current and force, and which type of force characteristic produces the greater reaction.

In the case of a single-phase fault on a 3-phase system it is necessary to obtain the value of the short-circuit current (I), either by actual tests or by means of the method of symmetrical components. From tests on a number of turbo-alternators it has been found that the single-phase line-to-line short-circuit current is approximately 85–100 per cent of the 3-phase short-circuit current.

The ratios of various types of single-phase fault currents to the symmetrical 3-phase fault current are given for modern alternators by Dannatt* in the following equations:—

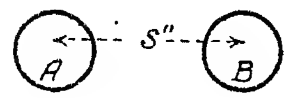
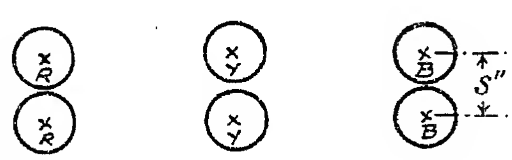
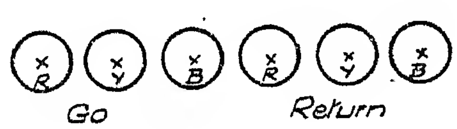
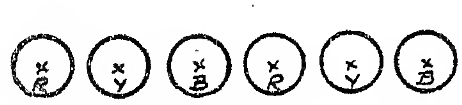
$$\frac{\text{Line-to-line short-circuit current}}{\text{Symmetrical 3-phase short-circuit current}} = \frac{\sqrt{3}Z_1}{Z_1 + Z_2}$$

* See Bibliography, (6).

$$\frac{\text{Line-to-neutral short-circuit current}}{\text{Symmetrical 3-phase short-circuit current}} = \frac{3Z_1}{Z_1 + Z_2 + Z_0}$$

$$\frac{\text{Two-lines-to-neutral short-circuit current}}{\text{Symmetrical 3-phase short-circuit current}} = \frac{Z_1[(a-1)Z_2 + (a-a^2)Z_0]}{(Z_0Z_1 + Z_1Z_2 + Z_2Z_0)}$$

TABLE 2.

Circuit	Arrangement of conductors	Maximum force	
		Conductor R	Conductor Y
Single-phase		$34 \cdot 8\, I^2/(10^7 s)$	
2-phase 3-wire Conductor R		$15 \cdot 2\, I^2/(10^7 s)$	
2-phase 3-wire Conductor Y		$35\, I^2/(10^7 s)$	
3-phase 3-wire Conductor R		$8 \cdot 65\, I^2/(10^7 s)$	
3-phase 3-wire Conductor Y		$26 \cdot 2\, I^2/(10^7 s)$	
3-phase 3-wire, two parallel leads Conductors R and Y		$13 \cdot 5\, I^2/(10^7 s)$* $6 \cdot 75\, I^2/(10^7 s)$†	$57\, I^2/(10^7 s)$* $28 \cdot 5\, I^2/(10^7 s)$†
3-phase 3-wire, go and return Conductors R and Y		$9 \cdot 8\, I^2/(10^7 s)$	$35\, I^2/(10^7 s)$
3-phase 3-wire, two parallel leads Conductors R and Y		$7 \cdot 8\, I^2/(10^7 s)$	$20 \cdot 8\, I^2/(10^7 s)$
3-phase 3-wire, two parallel leads Conductors R and Y		$4 \cdot 54\, I^2/(10^7 s)$	$53\, I^2/(10^7 s)$

* Horizontal component. † Vertical component.

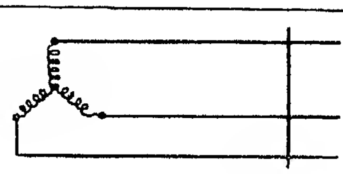
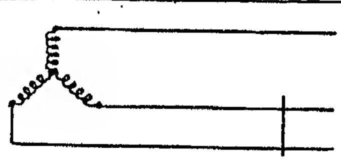
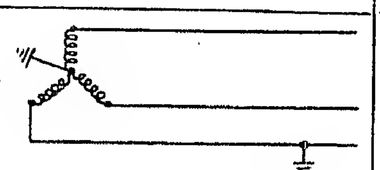
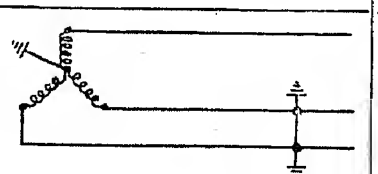
where Z_1 = positive-sequence reactance, Z_2 = negative-sequence reactance, Z_0 = zero-sequence reactance, and $a = -0{\cdot}5 + 0{\cdot}866j$.

The ratios for the particular case of an alternator with a transient impedance of $(0 + 100j)$ ohms are given in Table 3 and show that the single-phase short-circuit current is in general larger than the symmetrical 3-phase short-circuit current, and can reach over $1\frac{1}{2}$ times the latter. In the case of the symmetrical 3-phase short-circuit the following values, obtained from tests on modern machines with distributed poles were assigned: $Z_1 = 100$ ohms, $Z_2 = 70$ ohms, $Z_0 = 15$ ohms.

It is shown later that greater reactions are produced by a pulsating wave than by an alternating wave of the same amplitude. The most severe conditions are

TABLE 3.

Ratio of Single-Phase Short-Circuit Current to 3-phase Short-Circuit Current on a Typical Modern Alternator.

Type of fault		(Fault current) ÷ (Symmetrical 3-phase current—transient value)
Symmetrical 3-phase short-circuit		1·0
Single-phase, line-to-line		1·0
Single-phase, line-to-neutral		1·6
Single-phase, two-lines-to-neutral		1·4

therefore produced on a 3-phase system under the conditions of a single-phase fault.

(4) MECHANICAL REACTIONS TO ELECTROMAGNETIC FORCES.

Fundamental Principles.

The electromagnetic forces produced by alternating currents take the form of hammer blows of decreasing severity applied at regular intervals of time to the current-carrying conductors and their supports. The forces applied are of a cyclic character and either pulsate or alternate from a maximum in one direction to a maximum in the other, expending themselves in overcoming the rigidity of the supports and accelerating s mass.

In translating these electromagnetic forces into terms of mechanical reaction, it is useful to have some conception of the mechanism set in motion. Taking the case of the application of an alternating force, it would appear that the reactions produced are under control the whole time the current is flowing. The tendency for the energy stored in the structure to augment the reaction in one direction is to a considerable extent overcome by the reversal of current and applied force. When the current ceases, the whole of the energy stored in the structure is released and the damping of the structure is at the same time reduced.

The paradoxical condition then arises where the maximum applied force is coincident with the maximum value of current flowing, but the maximum reaction of the supports occurs at the instant the current starts and also at the instant that the current ceases to flow. It follows that the maximum reaction depends largely upon the point at which the force wave is interrupted by the cessation of current flow. If the applied force ceases at the instant it reaches a maximum value, the structure will have a maximum of stored energy available to produce the final reaction. On the other hand, should the applied force cease at the instant the force wave passes through zero, the structure will have a much smaller value of stored energy and the final reaction will be smaller.

The effects are reduced to some extent in practice by the fact that a well-designed oil circuit-breaker interrupts a current at or near its zero value. Tests have been carried out in this country and in America* which confirm the truth of this hypothesis. In these experiments it was shown that the application of a high-frequency alternating force to a non-resonating structure produces reactions at the instant the force is applied, and again as the force ceases, which are more than twice those obtained when the current is flowing. Considerable importance is attached to this fact, since it is not unusual for engineers to use in their calculations the r.m.s. value of current; they thus obtain the average force, and assume that the reactions corresponding to the maximum or peak values are never reached. The fact that the initial and final values of reaction exceed those obtained when the current is flowing supports the view that the maximum—and not the average—value of the force wave should be used, as is customary in America.

Reactions to Force Waves of Pulsating and Alternating Characteristic.

The reactions produced on a cantilever by force waves of pulsating and alternating characteristic were experimentally investigated by means of the apparatus shown in Fig. 13. The apparatus used for this purpose comprised a cantilever A, fixed at one end and with the other end free to vibrate between the poles of a permanent magnet D. The coil B, which surrounds the cantilever at the fixed end, was supplied with a constant value of current whose character was changed by means of half-wave and full-wave rectifiers. In this way alternating and pulsating forces of a constant amplitude were applied to the cantilever at line frequency. By means of a stroboscope the motion of the cantilever

* See Bibliography, (7).

under the action of the forces could be slowed down to any desired value, and in this manner the reactions of the cantilever to alternating and pulsating forces were observed and measured.

Tests were made over a wide range of frequency,

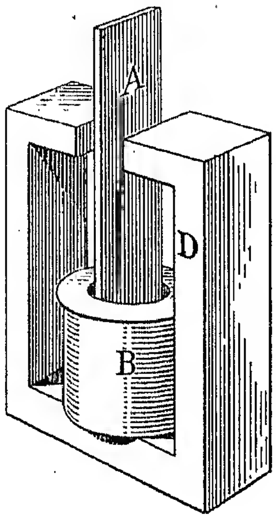


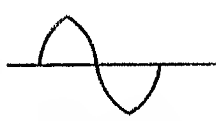
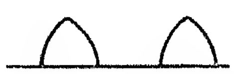
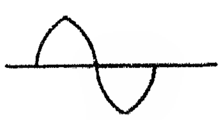
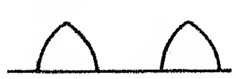
FIG. 13.

including the natural frequency of the cantilever itself, and all the reactions were observed under both resonating and non-resonating conditions. The results obtained are given in Table 4. They may be summarized as follows: (i) The reaction produced by the application of a pulsating force to the non-resonating cantilever approached double the reactions produced by an alternating force of similar frequency and amplitude. (ii) The effects of resonance were much more severe with a pulsating force than with an alternating force, and very quickly gave over 20 times the reaction normally obtained. (iii) The reactions were very sensitive to resonance; with an alternating force 5 cycles per sec. removed from the fully resonating position the reactions were reduced from 20 times to twice normal. (iv) Generally the pulsating force which characterizes all single-phase short-circuits and certain polyphase faults gave larger reactions than those produced by force waves of an alternating characteristic.

(5) APPLICATION OF RESULTS TO PRACTICAL CASES.

The application to practical cases of the results of this investigation into electromagnetic forces is carried out in an analytical manner by summing the various components of force exerted upon the system of conductors under consideration. It is not intended to deal with this aspect of the subject, since the treatment is simple and each case has to be considered on its own merits.

TABLE 4.

Test No.	Characteristic of force	Current	Frequency	Maximum reaction	Remarks
		amperes	cycles per sec.	inches	
1	 Full-wave rectification	0·4	25	$\frac{5}{16}$	Deflection greater on one side than the other
2	 Alternating	0·4	25	$\frac{3}{16}$	Deflection same on each side
3	 Half-wave rectification	0·4	25	$\frac{5}{16}$	Deflection greater on one side than the other
4	 Full-wave rectification	0·4	50	$\frac{1}{16}$	—
5	 Alternating	0·05	50	$\frac{1}{4}$	Resonance
6	 Half-wave rectification	0·05	50	$\frac{5}{16}$	Resonance

Important effects can, however, be observed from a general application of the results obtained, and certain of these relating to oil-switch contacts will now be referred to. Of the many examples that could be taken, the two which have been selected illustrate the importance of making a general survey of electromagnetic problems before commencing any detailed calculations or tests.

Forces between Oil-Switch Contacts.

The first example constitutes a general consideration of the forces exerted between different types of oil-switch contacts. The forces produced as the result of the current flowing from one contact to another are shown for various types of contacts in Fig. 14. In this figure the forces due to the proximity of other current-carrying conductors or other parts of the same conductor are ignored, and it is shown that there are a number of forces having an important effect upon the oil-switch contacts which are not generally taken into account.

The repulsive forces which are shown to act upon the various types of contacts tending to separate them have been actually experienced in practice. In certain oil circuit-breaker tests carried out recently these forces have been sufficient to separate the contacts under short-circuit conditions before the switch mechanism has had time to move. These forces have been experienced with each of the types of contacts shown, and will in fact always be present when the current in

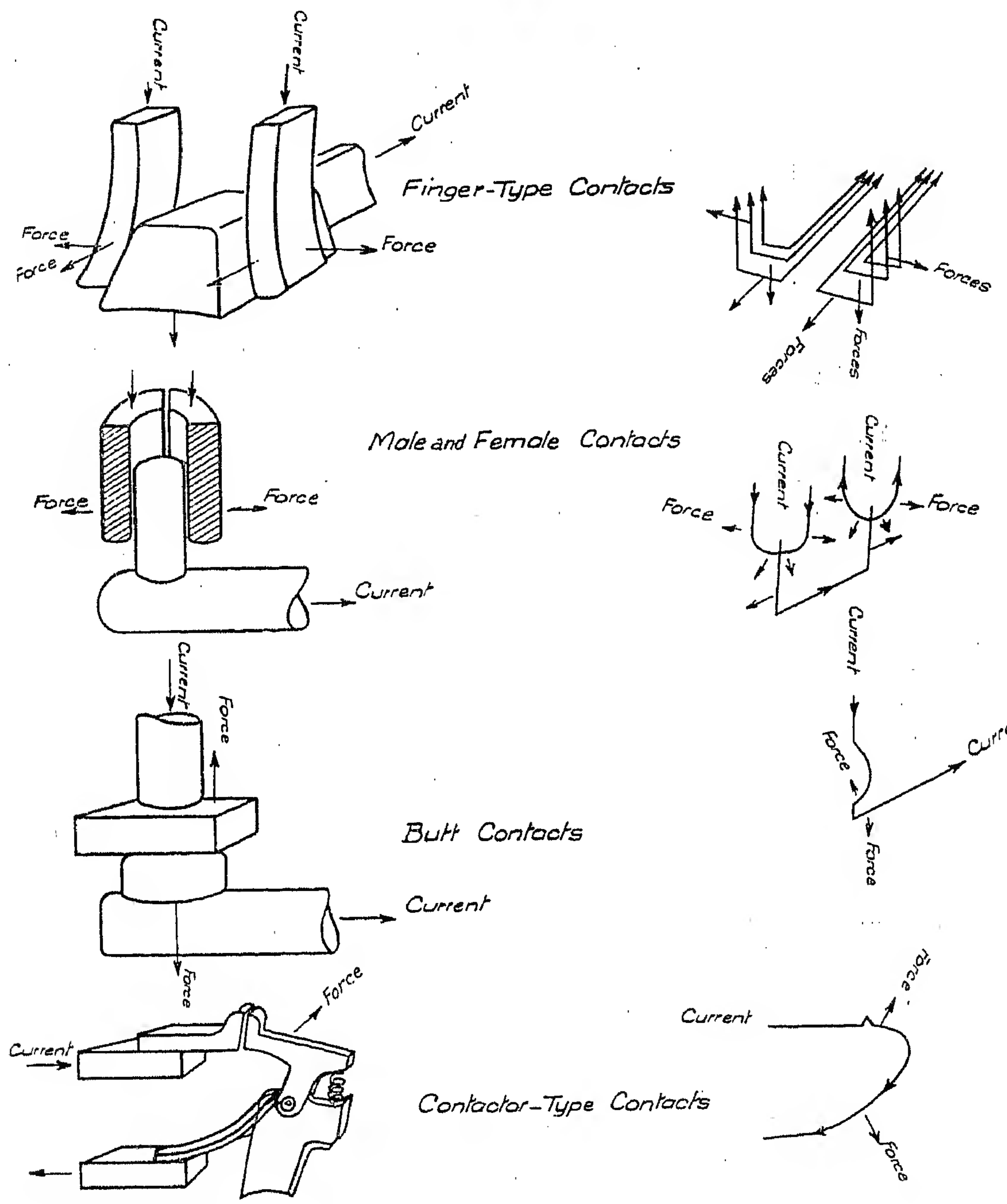


FIG. 14.—Forces produced by current flowing from one contact to another, ignoring other effects.

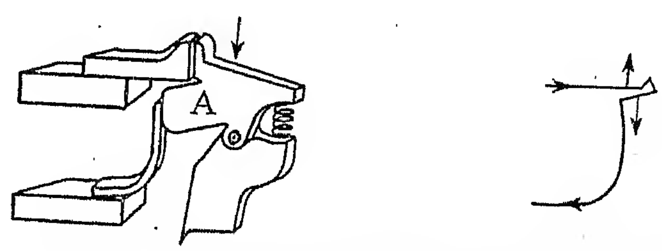


FIG. 15.—Modified design showing how contact separation was overcome.

passing from one contact to another has to make a change in direction.

There are other forces superimposed upon those shown, which have their origin in the proximity and

crowd effects, but these so far unrecognized forces of repulsion are capable of simple explanation in the way indicated.

Of all the simple types of contacts available, possibly the butt type (where the fixed and moving contacts are identical in shape and size) is from this point of view the best, since it allows the current to pass from one contact to another in a straight line and therefore no repulsive forces are produced.

It is, however, often possible to improve contacts in this respect by simple alterations to their shape, and this has been done in connection with contacts of the contactor type. Considerable difficulty was experienced with the contactor-type contacts shown in Fig. 14, owing to contact separation brought about when fault currents passed through them. This difficulty was completely overcome by redesigning the hinged portion A of the contacts in the manner shown in Fig. 15. The electromagnetic forces in the modified design were actually utilized to augment the contact pressure with the passage of large fault currents.

The effect produced on the forces by various arrangements of the contact fingers at the bottom of oil-switch terminal stems was also examined, and the results of this investigation are shown in Fig. 16. When the fixed contacts are placed in the space between the poles of the switch, as in Fig. 16(*c*), the blow-out force upon the arc is materially reduced. The maximum blow-out effect upon the arc is obtained when the contact fingers are placed in the space outside the poles, as in Fig. 16(*a*), where incidentally the forces are further increased by the proximity of the oil-switch tank.

These effects are dealt with in considerable detail in an article by A. L. Müller,* who claims that very considerable advantage is to be obtained in the breaking-capacity performance of oil switches by placing the

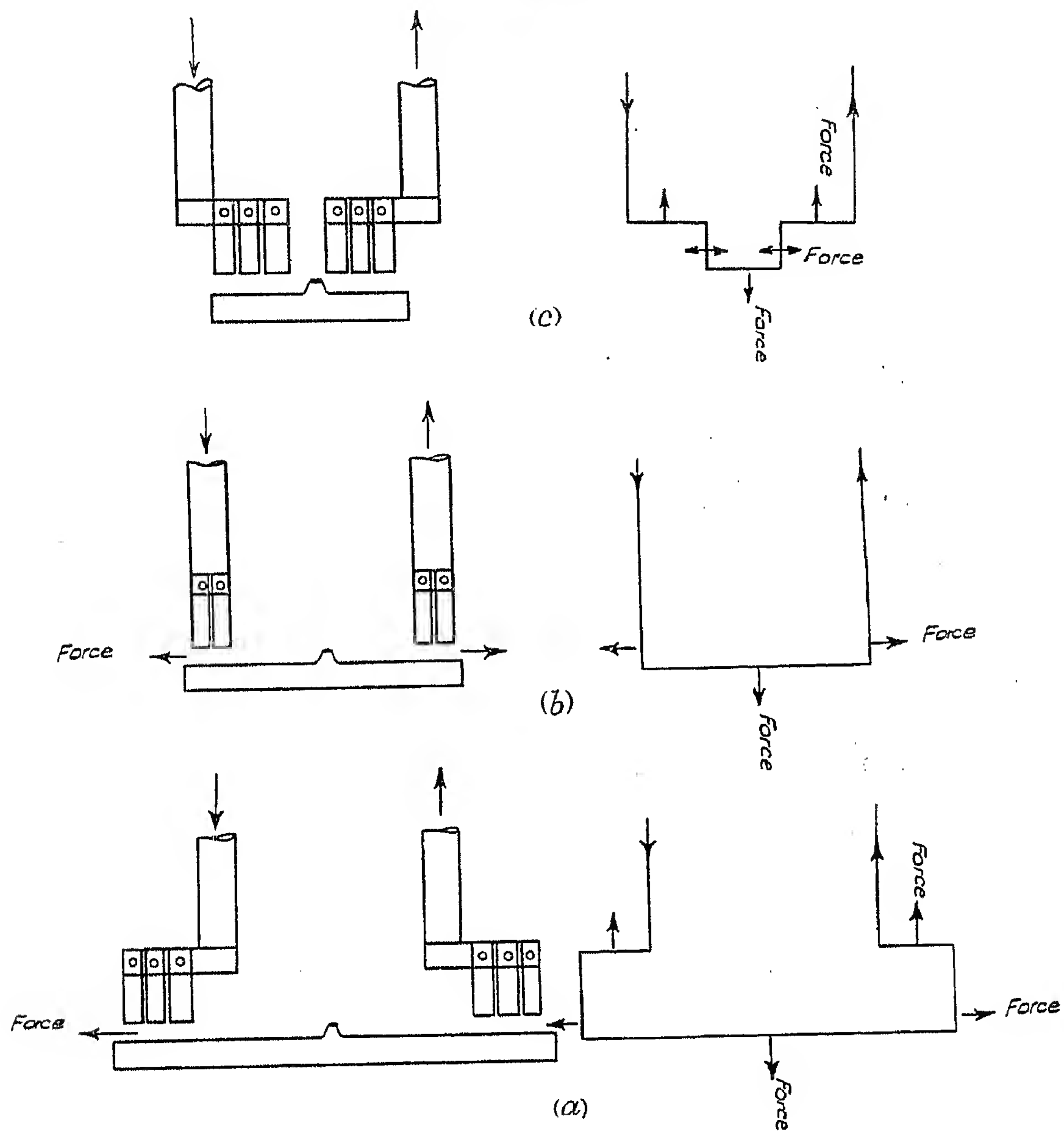


FIG. 16.

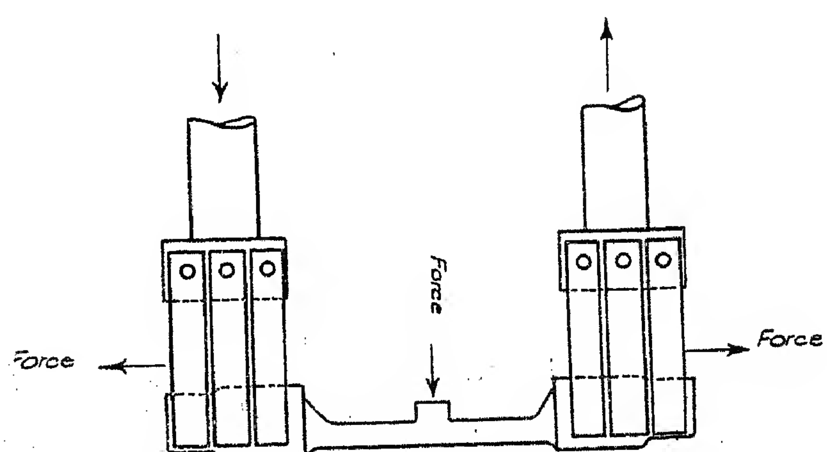


FIG. 17.—Forces exerted upon oil-switch loop.

* See Bibliography, (8).

finger contacts on the outside of the poles, as shown in Fig. 16(*a*). The added blow-out effect so obtained is stated to hasten the rupture of the arc by increasing the speed of movement of the arc root across the contact face, and also by increasing the cooling of the arc as it is forced through the oil.

Forces on Terminal Stems and Contact Bars of Oil Switches.

The second example of the effects of electromagnetic forces is concerned with the forces exerted upon the terminal stems and moving contact-bar of oil switches. These forces tend to increase the loop formed by the conductors, and in so doing to deflect the terminal stems and their support bushings and to move the contact bar in a downward direction. The forces exerted are shown in Fig. 17, and the probable reactions that will result upon the conductors in air when traversed by a single-phase and a 3-phase short-circuit current respectively are shown in Fig. 18.

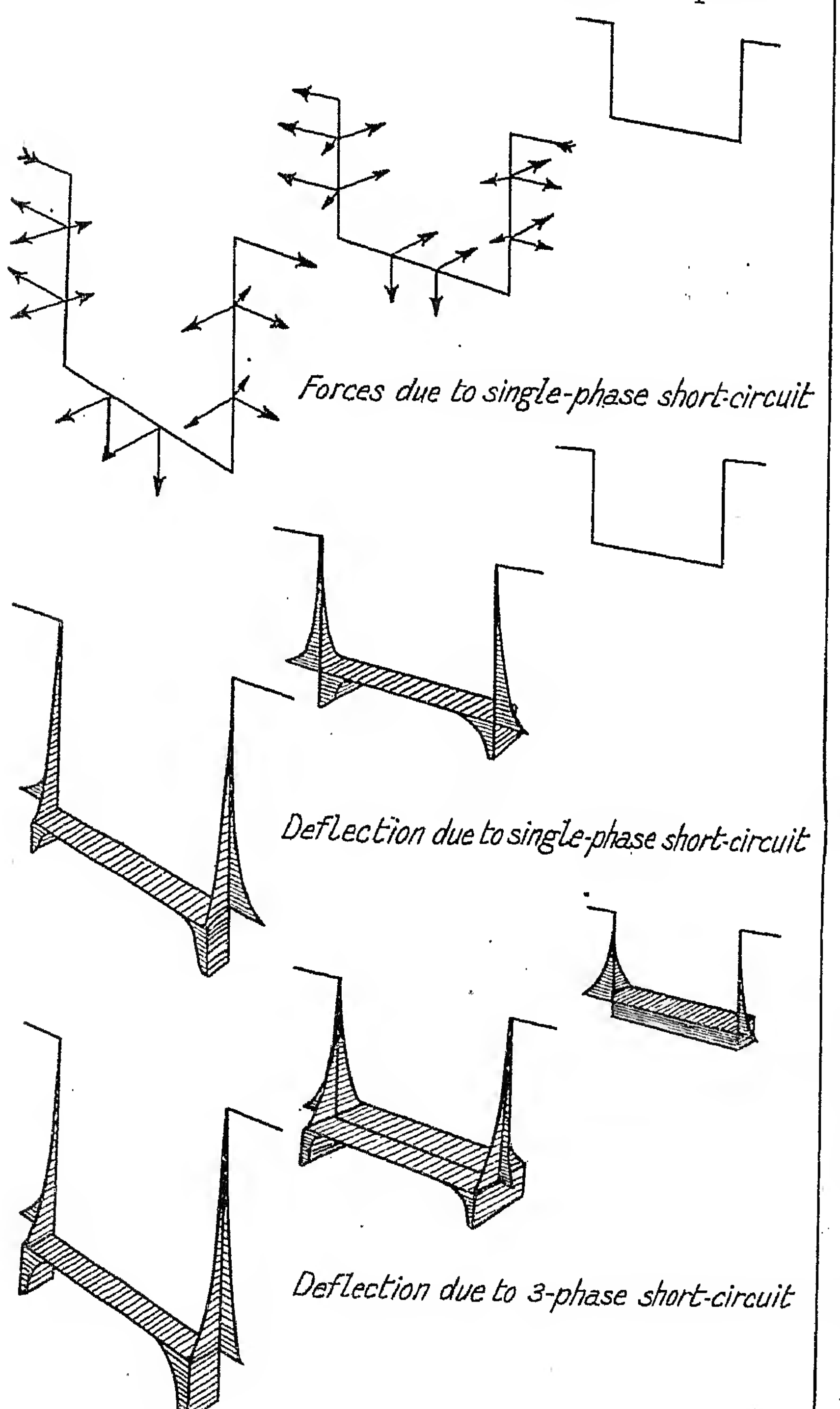


FIG. 18.

In the case of a single-phase short-circuit it is shown that the terminal stems of each of the affected poles of the breaker are deflected as a result of the vectorial sum of five distinct forces, and the reactions produced are consequently of a complex nature. The direction of movement of the terminal stems in their reactions to these forces is determined largely by the configuration of the conductors. From certain points of view the direction of the reaction is of greater importance than its magnitude, since the former may upset the bedding pressure of the contact fingers and so affect the breaking-capacity performance of the switch.

The magnetic top plate and the tank of the oil switch also have an effect upon the forces under consideration. The top plate tends to decrease the downward force on the cross-bar of the switch, but does not affect the expansive forces on the terminal stems and contacts to any extent. The steel tank, however, tends to increase both the downward and the expansive forces and is of considerable practical importance. In practice the presence of the top plate can be ignored, since its influence is negligible compared with that of the steel tank; and in any case, for heavy-current switches, non-magnetic top plates have to be used to reduce circulating currents round the terminals.

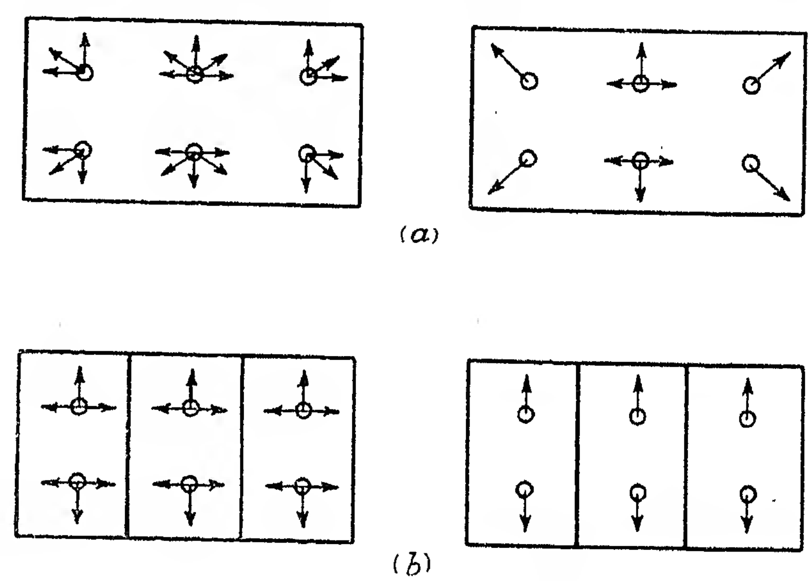


FIG. 19.—Effect of phase barriers.

(*a*) Without phase barriers.
(*b*) With phase barriers.

It is possible to calculate the increase in the downward and expansive forces upon the conductor loop on account of the presence of a steel tank when the current flowing in the conductors is unidirectional.

When an alternating current is used, however, such factors as the saturation of the steel tank, and its permeability, make even an approximation by calculation out of the question. The increase in the forces likely to result from the presence of the inductive tank can be comparatively easily determined by means of a simple series of tests. These should preferably be made on a rectangular loop of circular copper rod inserted into a steel tank, using the thickness of tank wall and size and disposition of contacts likely to be employed in practice. If the change in inductance of the loop is then measured for small displacements of each member of the rectangle, it is possible by Faraday's method to

deduce the increase in force brought about by the presence of the tank.

From a series of tests made in this manner it was found that the presence of the top plate could be safely ignored, while in the most severe case considered the presence of the oil-switch tank increased the downward and throw-off forces on the terminals, contacts, and cross-bar, to double the value they would have had in the absence of the tank.

The presence of phase barriers of inductive material in the tank has an important influence upon the magnitude and direction of the forces exerted upon the terminal stems and contacts of an oil switch under short-circuit conditions. The effect produced by these phase barriers is shown in Fig. 19. This indicates that when phase barriers are not used the forces exerted upon the terminal stems are such as to impart a twisting action to the finger contacts, which is most undesirable from the point of view of obtaining a good contact pressure. When the phase barriers are used, the forces exerted are greatly simplified and are of a type that do not affect the bedding pressure of most types of contacts. From this point of view, phase barriers of inductive material—quite apart from the inherent safeguard they offer against phase-to-phase faults—materially relieve the circuit breaker of certain undesirable forms of magnetic stress.

In conclusion, the author wishes to make full acknowledgment to all the authorities mentioned in the Bibliography, and to express thanks to the Metropolitan-Vickers Electrical Co. for permission to publish this paper.

BIBLIOGRAPHY.

(1) GRAY, A.: "Absolute Measurements in Electricity and Magnetism."

(2) HAGUE, B.: "Electromagnetic Problems in Electrical Engineering."

(3) DWIGHT, H. B.: "Calculation of Magnetic Force on Disconnecting Switches," *Transactions of the American I.E.E.*, 1920, vol. 39, p. 1337; "Calculation of Mechanical Forces in Electric Circuits," *ibid.*, 1927, vol. 46, p. 570; "Repulsion Between Strap Conductors," *Electrical World*, 1917, vol. 52, p. 522.

(4) BARROW, C. J.: *Transactions of the American I.E.E.*, 1911, vol. 30, p. 392.

(5) CLERC, A.: "The Reclosing on Short-Circuit of High-Power Circuit Breakers," *Revue Générale de l'Électricité*, 1928, vol. 24, pp. 217 and 255.

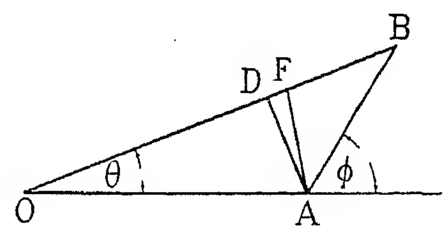
(6) DANNATT, C., and DALGLEISH, J. W.: "Electrical Power Transmission and Interconnection."

(7) VON ASPEREN, C. B.: "Mechanical Forces on Busbars under Short-Circuit Conditions," *Transactions of the American I.E.E.*, 1923, vol. 42, p. 1091.

(8) MÜLLER, A. L.: "The Duration of the Arc in an Oil Switch," *Archiv für Elektrotechnik*, 1930, vol. 24, p. 503.

DISCUSSION ON "THE MEASUREMENT OF IMPEDANCE."*

Mr. E. V. Clark (Australia) (*communicated*): The method of measuring impedance described by the author is in principle, though not in apparatus, closely analogous to a method given by C. G. Lamb.† In this, a non-inductive resistance is placed in series with the coil under test, and in parallel with the two is connected a resistance, with sliding contact, that need not be non-inductive provided that it is of uniform time-constant throughout its length. The vector diagram is shown in the Figure. OA is the voltage across the non-inductive resistance, AB that across the coil, and OB that across the resistance of uniform time-constant. These are measured by a voltmeter; and this is then connected between point A and the sliding contact D, which is so adjusted (extreme accuracy being unnecessary) that the voltmeter reading is a minimum when DA is perpendicular to OB and $\sin\theta = AD/OA$. The author



obtains the same vector diagram, except that instead of point D, he finds point F such that $OF = OA$, whereupon $\sin\frac{1}{2}\theta = AF/(2 \times OA)$. Lamb's method is ostensibly to find the phase angle of the coil; but by using a non-inductive resistance of known value, the impedance, reactance, and resistance of the coil are readily determined from equations analogous to those which the present author gives.

This older method devised by Lamb avoids the necessity of using an accurately graduated resistance, but requires the voltages to be read directly. The author's method has the advantage that an uncalibrated voltmeter or its equivalent may be used, as one virtually calibrates it as one goes along upon the graduated resistance. One would surmise that herein lies the chief drawback of the method; that the limits to its accuracy would be found in the measuring of a voltage such as OF to agree exactly with a previously measured voltage, OA.

* Paper by Mr. N. F. Astbury (see vol. 74, p. 445).
† "Alternating Currents," (1906), p. 40.

Would not much greater accuracy be possible if, instead of using a normal voltmeter or its equivalent, one constructed a differential voltmeter, one coil of which was excited by the voltage across OF and the other by that across OA? This would permit of determining the tapping points such as F by a null method, and the sensitivity of the detecting instrument might be as high as desired. A differential galvanometer, with resistance of the order of a megohm and a rectifying valve in series with each winding, should prove satisfactory. It would, of course, be necessary to match the valves very carefully; but a change-over switch would eliminate errors from want of perfect matching, or at any rate indicate the order of the inaccuracy due to this cause.

Nevertheless, however sensitive the apparatus used, the method is inherently of poor accuracy when one is endeavouring to measure the resistance of an impedance coil of large time-constant; since the expression for this resistance, $R = (r/R_1)R_P \cos\theta - r$, is in these circumstances palpably the small difference between two large quantities.

Mr. N. F. Astbury (*in reply*): I was not previously acquainted with the method of C. G. Lamb, described by Mr. Clark. My method in its simplest form is, of course, only a development of the traditional "3-voltmeter" method for measuring power, and it is not surprising that the further possibilities of this method should have been realized by others.

I feel that the limitations of my method are fairly well exemplified by the measurements recorded in the paper, and I would refer Mr. Clark to them. He will find ample support for his last paragraph, especially in the measurements on condensers, which were made with the express purpose of showing up the limitations of the method. In such cases as Mr. Clark mentions, however, the main interest is usually, although not invariably, in the reactive component.

Mr. Clark's suggestion for a differential voltmeter is extremely interesting. If higher accuracy is required, however, surely the use of an ordinary a.c. bridge would be the best way of achieving this result. The essence of my method, I had hoped, was its simplicity: to detract from this without at the same time greatly increasing the accuracy seems hardly worth while.

INSTITUTION NOTES.

Overseas Members and the Institution.

During the period 1st July to 30th September, 1934, the following members from overseas called at the Institution and signed the "Attendance Register of Overseas Members":—

Beckett, T. R., B.Sc. (Eng.), (*Lagos*).
Beynon, J. H. (*Paulsboro, U.S.A.*).
Cooney, Rev. T., S.J., B.Sc.(Eng.), Ph.D. (*Hong Kong*).
de Aguiar, J. (*Rio de Janeiro*).
Del Mar, W. A. (*Greenwich, U.S.A.*).
Fenton-Jones, H. (*Colombo*).
Geare, H. W. (*De Hoek, S. Africa*).
Gordon, A. H. (*Shanghai*).
Harmer, L. B. (*Shanghai*).
Higham, R. G. (*Bombay*).
Mellor, A. H. (*Johannesburg*).
Migotti, L. W. (*Buenos Aires*).
Mountain, G. A., B.Sc. (Eng.), (*Hamilton, Bermuda*).
Ogden, Captain G. W., B.A., R.S. (*Khartum*).
Parsons, D. B., M.Eng. (*Buenos Aires*).
Purcell, B. G. (*Calcutta*).
Selvey, A. M. (*Detroit*).
Shanahan, D., B.E., B.Sc. (*Baghdad*).
Sharpley, Prof. F. W., F.R.S.E. (*Dhanbad, India*).
Swingler, G. H. (*Cape Town*).
Wales, Prof. W. A., B.Sc. (Eng.), (*Madras*).
Worth, T. C., B.A. (*Madras*).

Members from Overseas.

The Secretary will be obliged if members coming home from overseas will inform him of their addresses in this country, even if they do not desire a change of address recorded in the Institution register.

The object of this request is to enable the Secretary to advise such members of the various meetings, etc., of the Institution and its Local Centres, and, when occasion arises, to put them into touch with other members.

Proceedings of the Meter and Instrument Section.

43rd Meeting of the Meter and Instrument Section, 2nd March, 1934.

Mr. W. Lawson, Chairman of the Section, took the chair at 7 p.m.

The minutes of the meeting held on the 2nd February, 1934, were taken as read and were confirmed and signed.

The following papers were read and discussed:— "Copper-Oxide Rectifiers in Ammeters and Voltmeters" (see page 453), by Mr. E. Hughes, D.Sc., Ph.D., Member; and "A Direct-Reading Form Factor Meter" (see page 463), by Mr. R. S. J. Spilsbury, B.Sc.(Eng.), Member.

The meeting terminated at 9.17 p.m. with a vote of thanks to the authors, which was moved by the Chairman and carried with acclamation.

44th Meeting of the Meter and Instrument Section, 16th March, 1934.

Mr. W. Lawson, Chairman of the Section, took the chair at 7 p.m.

The minutes of the meeting held on the 2nd March, 1934, were taken as read and were confirmed and signed.

An informal discussion took place, the subjects being as follows:—

(1) "The Measurement of Current in Substations" (opened by Mr. F. E. J. Ockenden, Associate Member).

(2) "Three-Phase Four-Wire Meters: Two Elements or Three?" [opened by Mr. R. S. J. Spilsbury, B.Sc.(Eng.), Member].

(3) "Protection Methods for Three-Phase Transformer Banks" (opened by Mr. O. Howarth, Member).

The meeting terminated at 9.8 p.m. with a vote of thanks, moved by the Chairman, to those members who had introduced the subjects for discussion.

45th Meeting of the Meter and Instrument Section, 13th April, 1934.

Mr. W. Lawson, Chairman of the Section, took the chair at 7 p.m.

The minutes of the meeting held on the 16th March, 1934, were taken as read and were confirmed and signed.

A paper by Mr. G. F. Shotter, Associate Member, entitled "Experience with, and Problems relating to, Bottom Bearings of Electricity Meters," was read and discussed.

The meeting terminated at 9.55 p.m. with a vote of thanks to the author, which was moved by the Chairman and carried with acclamation.

46th Meeting of the Meter and Instrument Section, 4th May, 1934.

Mr. W. Lawson, Chairman of the Section, took the chair at 7 p.m.

The minutes of the meeting held on the 13th April, 1934, were taken as read and were confirmed and signed.

The Chairman announced that the following members had been nominated to fill the vacancies which would occur on the Committee on the 1st October, 1934:—

Chairman: Prof. J. T. MacGregor-Morris.

Ordinary Members of Committee: B. S. Cohen, O.B.E., S. James, G. F. Shotter, H. Cobden Turner, and J. G. Wellings.

In the event of a ballot for the new Committee being required, Messrs. H. Honey and J. F. Shipley were appointed scrutineers.

Prof. W. Cramp, D.Sc., Member, then delivered a Lecture entitled "An Experimental Sequel to Faraday's Work of 1831."

A vote of thanks to the lecturer, proposed by Prof. J. T. MacGregor-Morris and seconded by Mr. F. E. J. Ockenden, was carried with acclamation.

The meeting terminated at 8.30 p.m.

Accessions to the Reference Library.

[NOTE.—The books cannot be purchased at the Institution; the names of the publishers and the prices are given only for the convenience of members; (*) denotes that the book is also in the Lending Library.]

REES, H. Worked examples for wiremen and students. sm. 8vo. vii + 114 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1933.) 3*s.* 6*d.* (*)

ROTHE, R., OLLENDORFF, F., and POHLHAUSEN, K. Theory of functions as applied to engineering problems. Transl. by A. Herzenberg. 8vo. x + 189 pp. (Cambridge: Massachusetts Institute of Technology, 1933.) $3.50.

SCHALL, W. E. X-rays: their origin, dosage, and practical application. 4th ed. la. 8vo. xi + 181 pp. (Bristol: John Wright and Sons, Ltd., 1932.) 7*s.* 6*d.*

SCHALLREUTER, W. L., *Dr.Ph.* Neon tube practice. la. 8vo. 132 pp. (London: Blandford Press, Ltd., 1933.) 10*s.* 6*d.* (*)

SCHÜLE, W., *Dipl.-Ing.* Technical thermodynamics. Transl. by E. W. Geyer. 8vo. xv + 627 pp. + 3 pl. (London: Sir Isaac Pitman and Sons, Ltd., 1933.) 40*s.* (*)

STOCKTON, R. C. The principles of electric welding: metallic arc process. sm. 8vo. vii + 184 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1933.) 7*s.* 6*d.* (*)

STODDART, W. C. The working principles of motor vehicle lighting and starting. 8vo. xv + 504 pp. (London: "Automobile Electricity," 1933.) 15*s.* (*)

STRANGER, R., *pseudonym.* Dictionary of wireless terms. 8vo. viii + 160 pp. (London: George Newnes, Ltd., 1933.) 2*s.* 6*d.*

—— The elements of wireless. 8vo. xi + 212 pp. (London: George Newnes, Ltd., 1933.) 3*s.* 6*d.* (*)

TAYLOR, D. Power sales. sm. 8vo. xii + 206 pp. (New York, London: McGraw-Hill Book Co., Inc., 1933.) 12*s.* 6*d.* (*)

TEMPLE, G., *Ph.D.*, *D.Sc.* The general principles of quantum theory. sm. 8vo. viii + 120 pp. (London: Methuen and Co., Ltd., 1934.) 3*s.*

TODMAN, J. C. Power economy in the factory. A book for cost and works accountants, and also for students intending to enter for the Power Generation and Transmission Section of the Examination of the Institute of Cost and Works Accountants. sm. 8vo. (London: Sir Isaac Pitman and Sons, Ltd., 1932.) 7*s.* 6*d.* (*)

UNDERHILL, C. R. Electrons at work. A simple and general treatise on electronic devices, their circuits, and industrial uses. 8vo. xii + 354 pp. (New York, London: McGraw-Hill Book Co., Inc., 1933.) 18*s.* (*)

VALENSI, G. Théorie de la transmission téléphonique. 8vo. 626 pp. (Paris: Léon Eyrolles, 1933.) 80 *francs.*

VERBAND DEUTSCHER ELEKTROTECHNIKER. Vorschriftenbuch. Herausgegeben durch das Generalsekretariat des V.D.E. 19e Aufl. 8vo. xvi + 1271 pp. (Berlin: Verlag des Verbandes Deutscher Elektrotechniker, 1933.) *RM.* 16.20.

VYVYAN, R. N. Wireless over thirty years. 8vo. xiv + 256 pp. (London: George Routledge and Sons, Ltd., 1933.) 8*s.* 6*d.* (*)

WAGNER, C. F., and EVANS, R. D. Symmetrical components as applied to the analysis of unbalanced electrical circuits. With an introduction by C. L. Fortescue. 8vo. xvi + 437 pp. (New York, London: McGraw-Hill Book Co., Inc., 1933.) 30*s.* (*)

WALKER, M., *M.A.*, *D.Sc.*, *F.R.S.* Conjugate functions for engineers. A simple exposition of the Schwarz-Christoffel transformation applied to the solution of problems involving two-dimensional fields of force and flux. la. 8vo. 116 pp. (Oxford: University Press, 1933.) 12*s.* 6*d.* (*)

WALKER, R. C., and LANCE, T. M. C. Photoelectric cell applications. 8vo. viii + 193 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1933.) 8*s.* 6*d.* (*)

WATT, R. A. W., HERD, J. F., and BAINBRIDGE-BELL, L. H. Applications of the cathode ray oscillograph in radio research. (Dept. of Scientific and Industrial Research). 8vo. xvi + 290 pp. (London: H.M. Stationery Office, 1933.) 10*s.* (*)

WEINBACH, M. P. Alternating current circuits. 8vo. xvi + 417 pp. (New York: The Macmillan Co., 1933.) 20*s.* (*)

WHITE, F. W. G., *Ph.D.* Electromagnetic waves. sm. 8vo. viii + 108 pp. (London: Methuen and Co., Ltd., 1934.) 3*s.* (*)

WILDBORE, H. J. W. Patents explained. 8vo. 48 pp. (London: J. B. Wildbore and Son and John A. Gibbons and Co., 1933.) 5*s.*

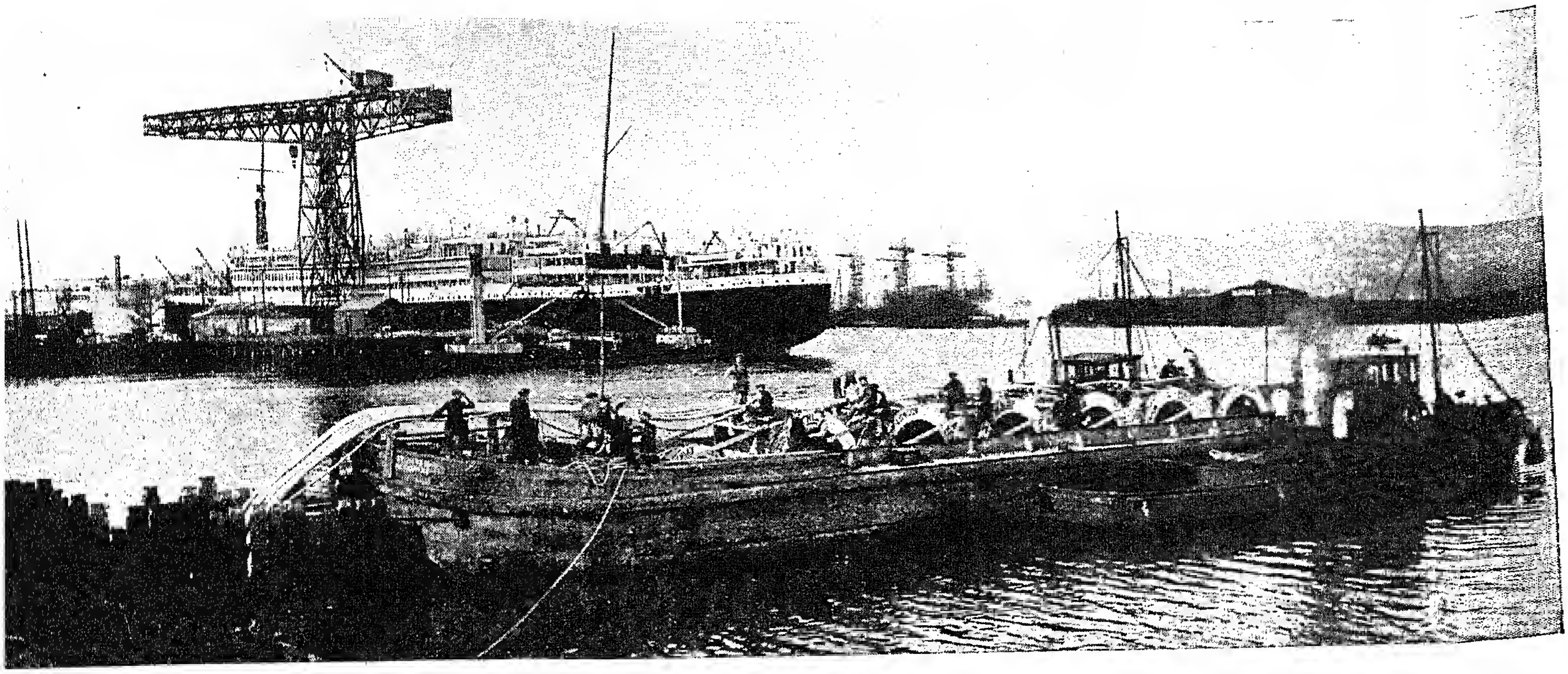
WILSON, W., *M.Sc.* The calculation and design of electrical apparatus. sm. 8vo. xv + 214 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1934.) 10*s.* 6*d.*

WITTS, A. T. Radio upkeep and repairs for amateurs. A practical handbook on fault-clearing and set maintenance. 8vo. ix + 158 pp. (London: Sir Isaac Pitman and Sons, Ltd., 1933.) 5*s.* (*)

WORLD PETROLEUM CONGRESS, 1933. Proceedings. Edited by A. E. Dunstan and G. Sell. vol. 1, Geological and production sections, xxiv + 592 pp., 35*s.*; vol. 2, Refining, chemical and testing section, xxvi + 956 pp. 45*s.*; 2 vol., 73*s.* 6*d.* (London: World Petroleum Congress, 1934.)

YOUNG, A. P., *O.B.E.* Forward from chaos. 8vo. xvi + 188 pp. (London: Nisbet and Co., Ltd., 1933.) 6*s.*

YOUNG, A. P., and GRIFFITHS, L. Automobile electrical equipment. 8vo. 336 pp. (London; Iliffe and Sons, Ltd., 1933.) 15*s.* (*)



Crossing the Channel

The laying of six submarine cables across the Victoria Channel between the East and West Twin Islands at Belfast was recently carried out by Henley's in connection with the supply for the Antrim side of the city from the City Council's power house.

The cables were 1 sq. in. 35,000 volt, 3-core S.L. double wire armoured with reinforcing strands, together with a 12 pair pilot and telephone cable, paper insulated, lead covered and single wire armoured.

Preliminary operations are illustrated in the top picture, and the circle shows the barge leaving the East Bank of the Channel.

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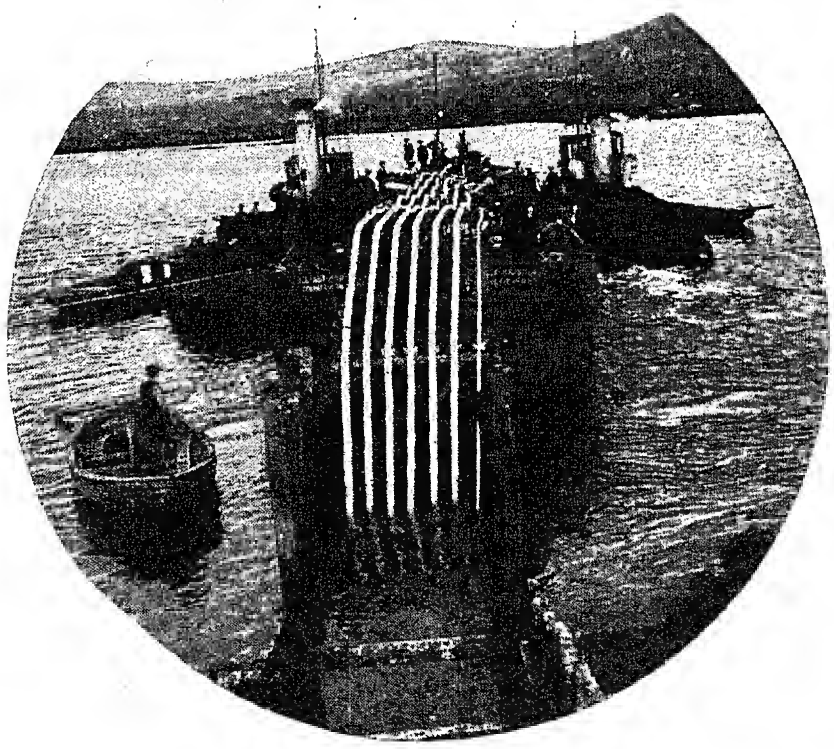

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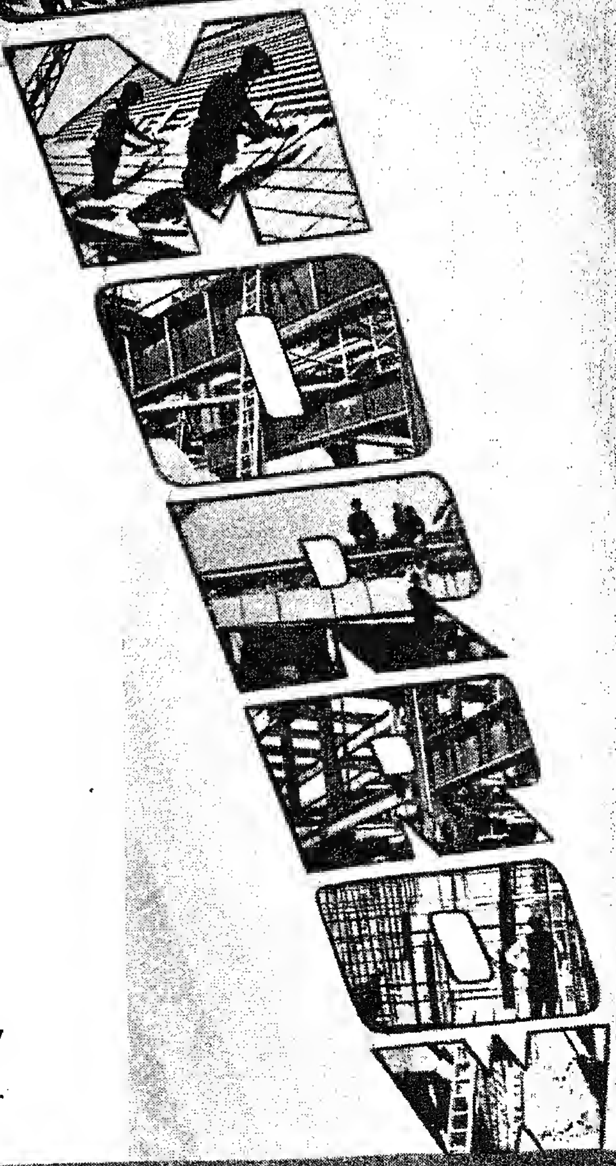

The illustration is of a 182 kVA B.I. Condenser fitted with H.T. Control Switch; 2,000V, 50 Cycles, 2 Phase 3 Wire; supplied to Messrs. Metropolitan Cammel Carriage & Wagon Company.

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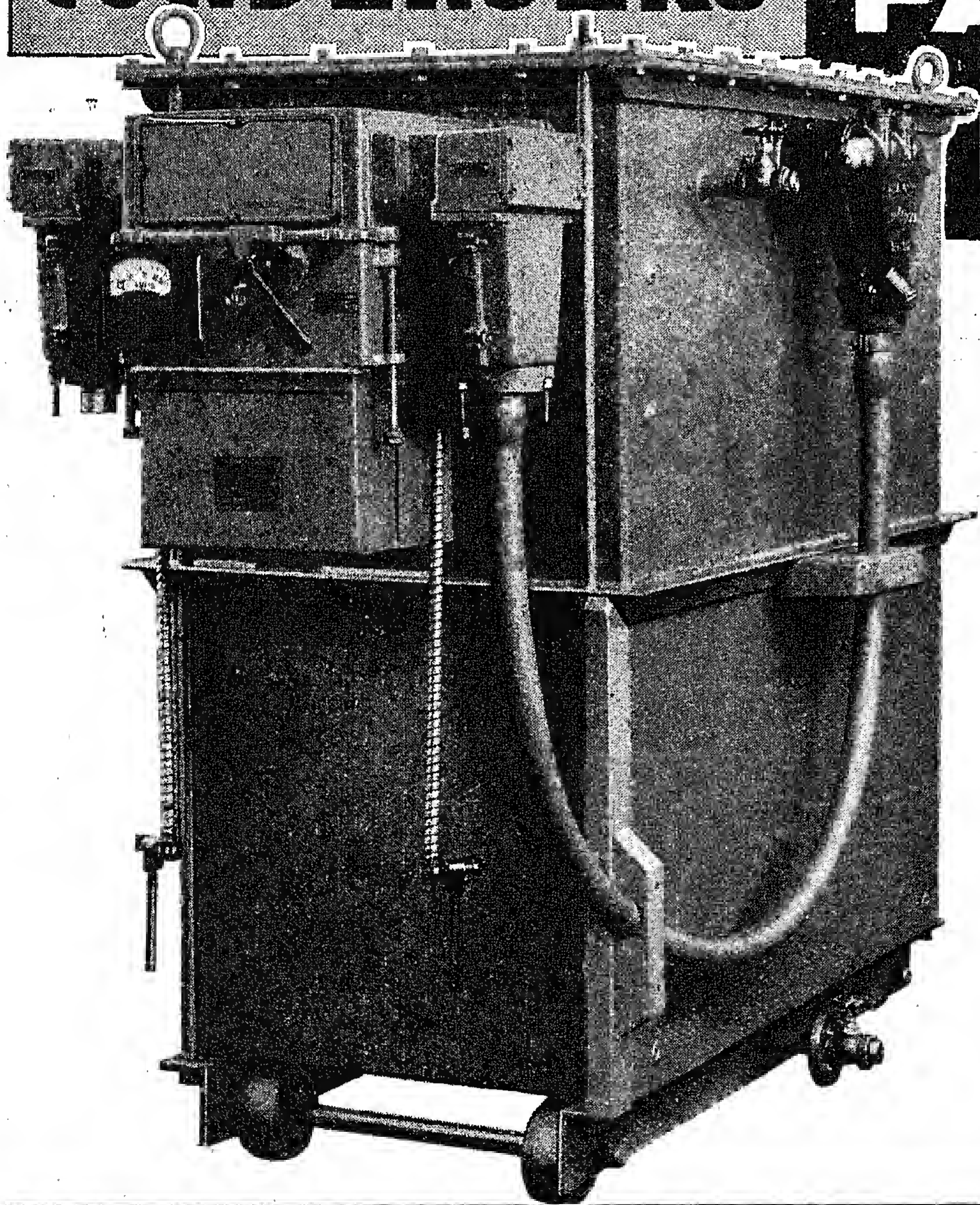

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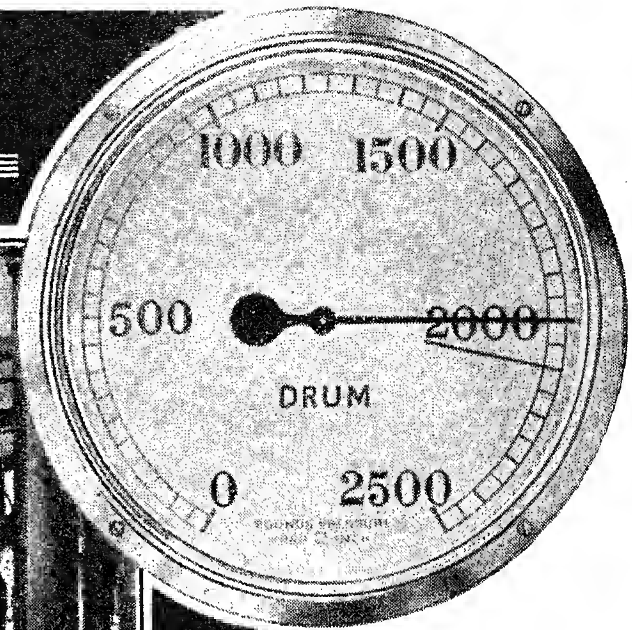

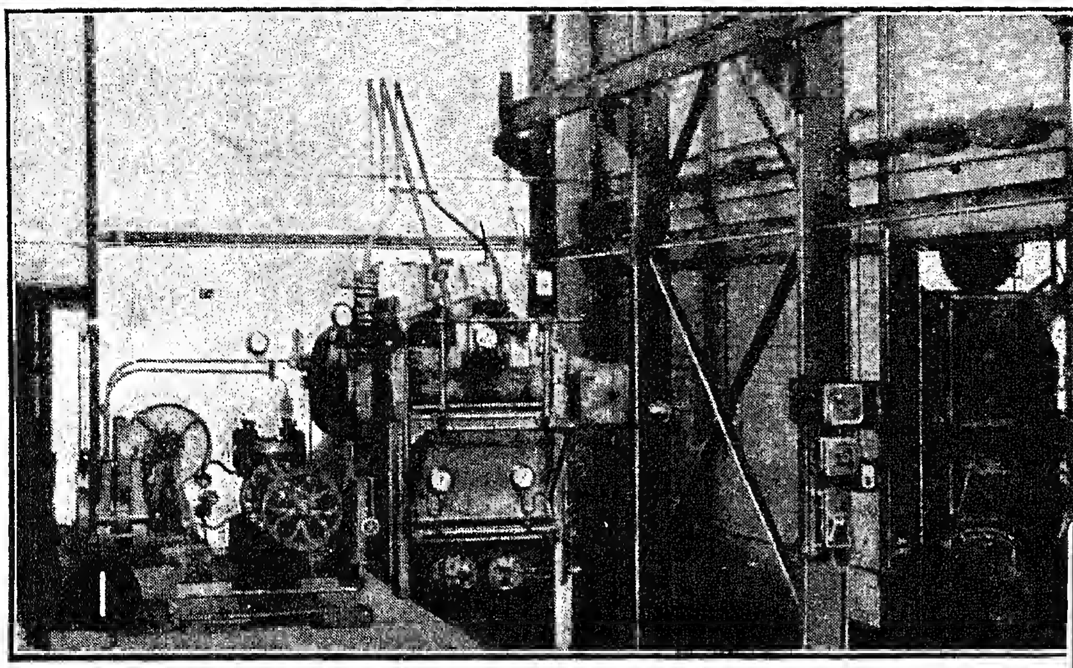

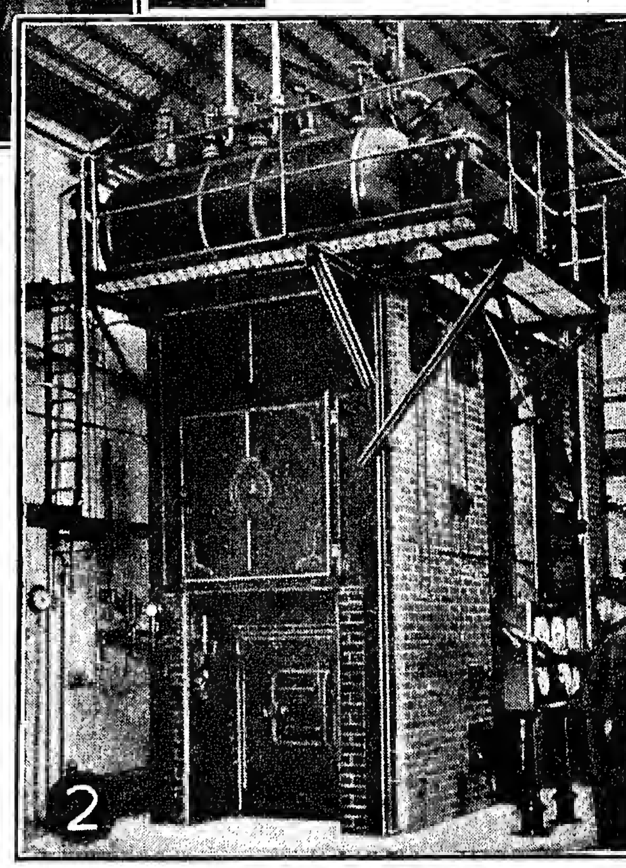

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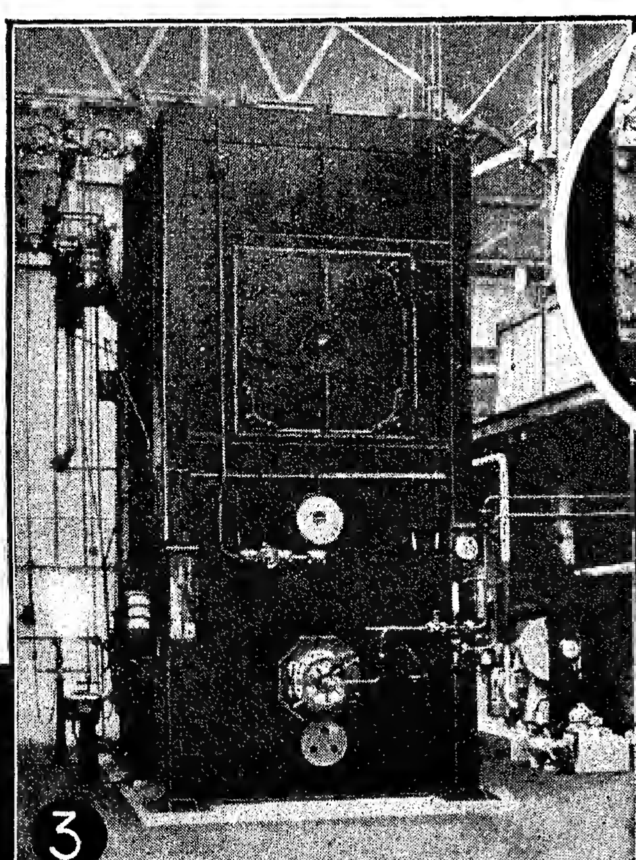
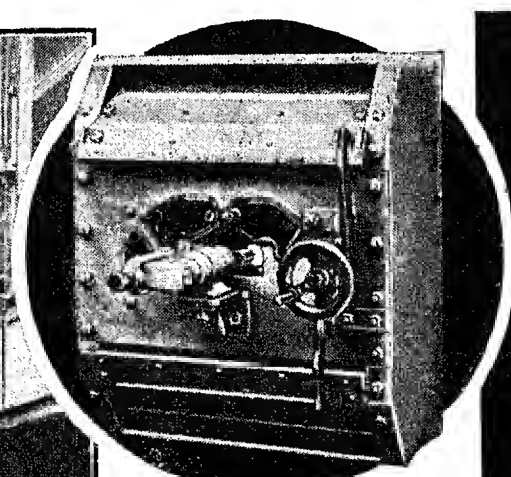
2 Dewrance & Co., London. An oil fired B. & W. Boiler with a capacity of 2,000 lbs. of steam per hour, the drum safety valve being set at 2,140 lbs. per square inch pressure and final steam temperature of 875°F., installed in 1933. This boiler is also fitted with an economiser.

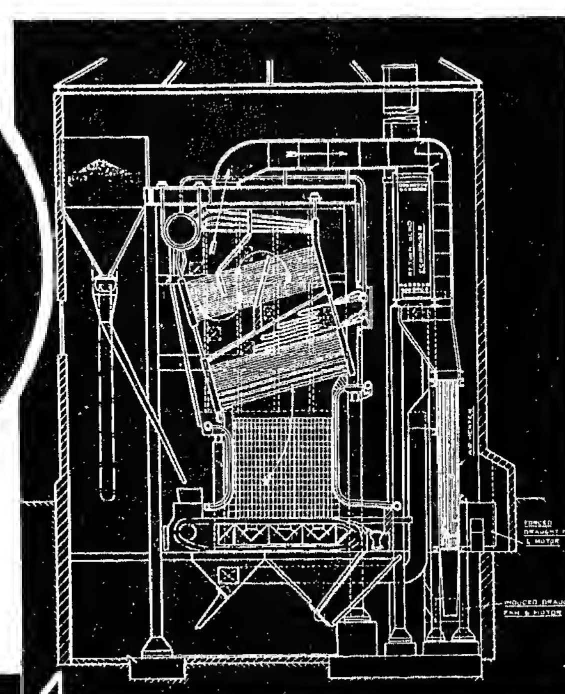
3 Hopkinsons Ltd., Huddersfield. An oil fired B. & W. Boiler with a capacity of 2,000 lbs. per hour at 2,000 lbs. per square inch and 850°F. This unit was installed in 1931.

4 A test boiler on a commercial scale installed in our Renfrew Works. This boiler is capable of a load of 40,000 lbs. of steam per hour, at 1575 lbs. per square inch pressure and 840°F. It is fired by means of a B. & W. Style 28 Stoker in a furnace of Bailey construction. This boiler was installed in 1929.





In all cases the oil firing equipment is by B. & W., and above is shown our present design of oil burner air control box with the safety dampers which shut automatically in the event of a blow-back in the furnace.



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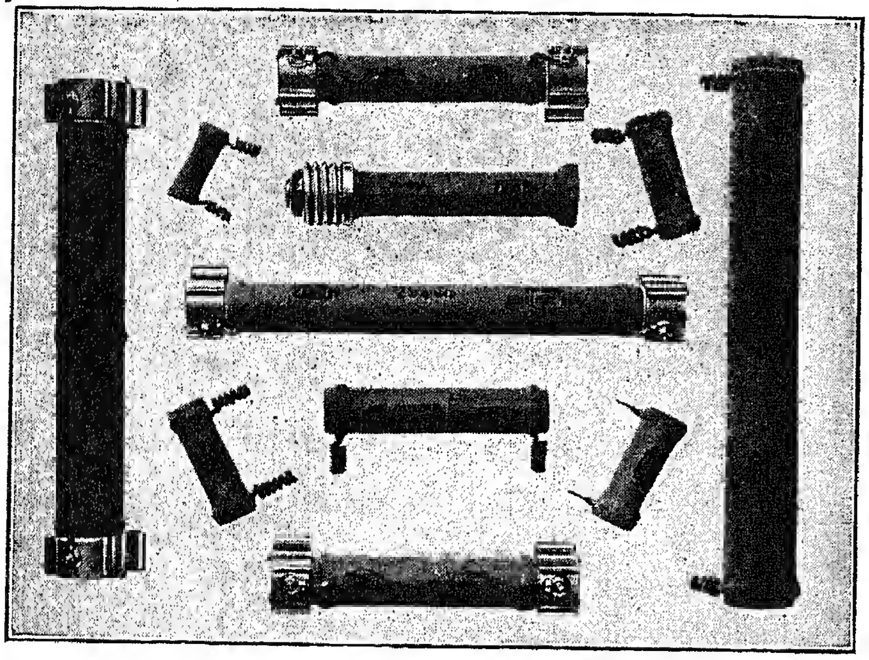

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*0-1.2 "	*0- 120 "	*0- 10,000 "	0- 1.2 "	0- 480 "
0- 600 m.a.	0- 60 "	*0- 1,000 "	0- 0.6 "	0- 240 "
*0- 120 "	*0- 12 "		0-120 milliamps.	0- 120 "
0- 60 "	0- 6 "		0- 60 "	0- 60 "
*0- 12 "	*0- 1.2 "			0- 12 "
0- 6 "	0- 600 millivolts.			0- 6 "
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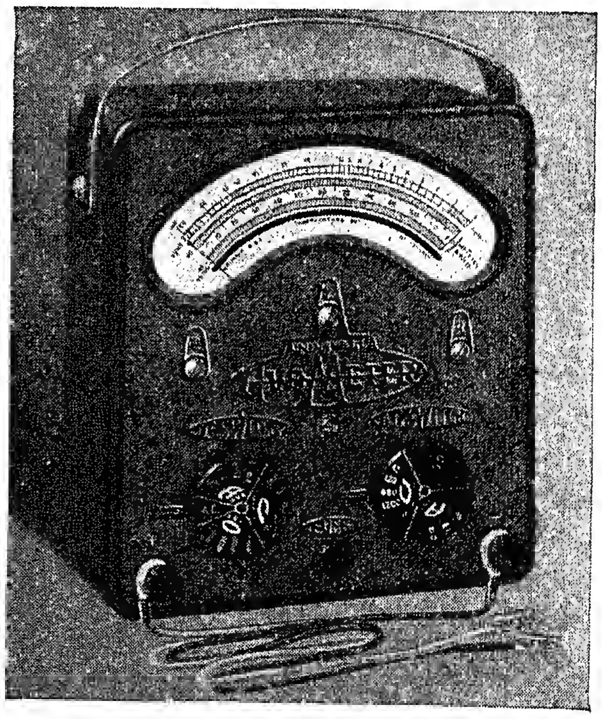

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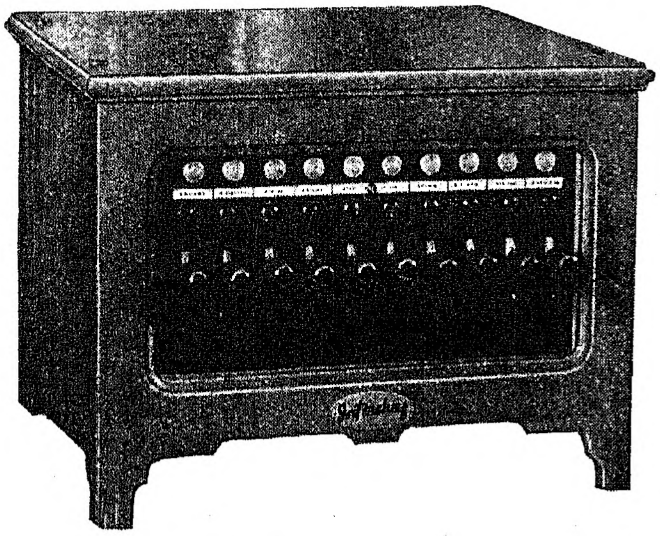

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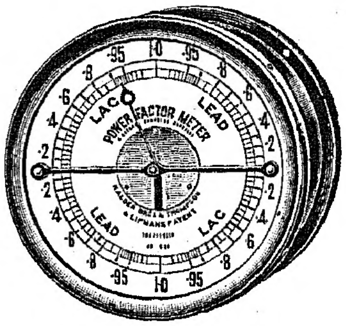

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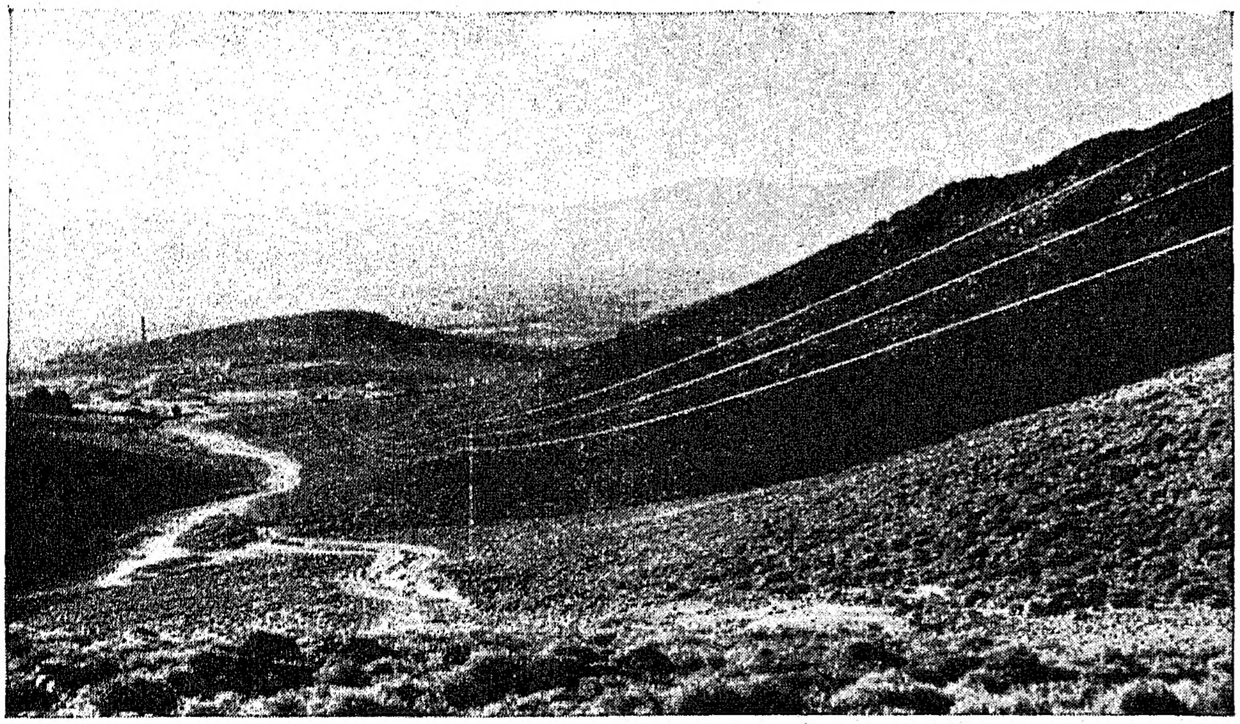

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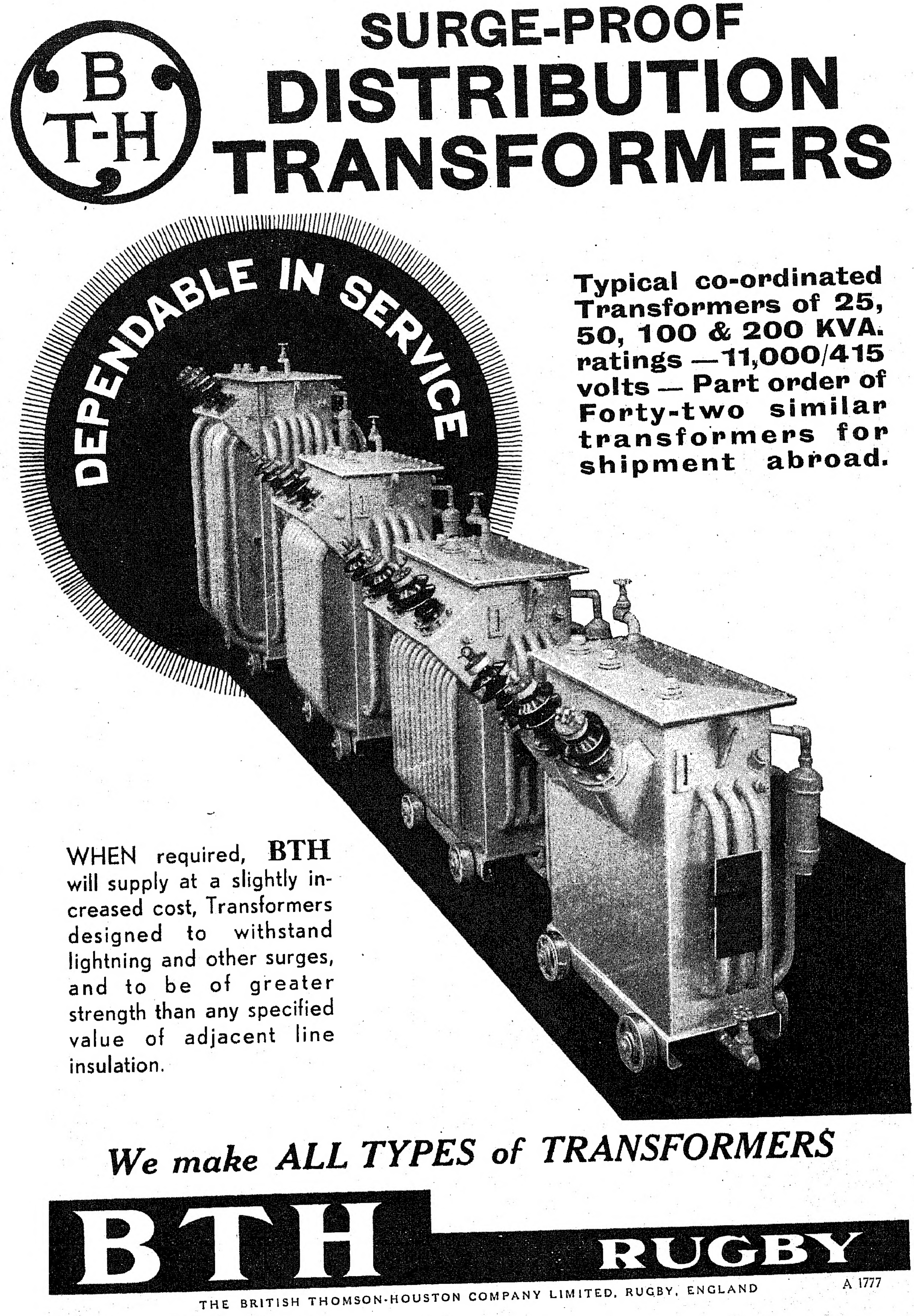
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